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The Place of Principles in Policy Analysis*

CHARLES W. ANDERSON
University of Wisconsin, Madison

Any theory of policy evaluation has to address the problem of the choice of criteria for decision making. In most theories of policy rationality, derived from economic theories of the utility-maximizing individual and a positivist conception of valuation, such values are to be regarded as the "preferences" of the policy maker. The stipulation and ordering of standards of judgment is not considered to be part of policy rationality itself. This conception of rationality is not obligatory. Understanding rationality as having good reasons for an action, and policy judgment as a process of argument, enables us to stipulate certain standards at the metapolitical level which any system of policy evaluation must meet. It is possible to identify a logical sense in which such classic principles as authority, justice and efficiency can be understood as necessary considerations in any rationally defensible policy appraisal.

In order to make a policy decision, one must invoke some criteria of evaluation. We cannot decide whether a proposal for public action is desirable or undesirable, whether the results of a public program are to be adjudged a success or a failure, except in the light of a standard. We have to make a decision on some basis, for some reason. We can favor a specific program for the reason that the benefits seem to outweigh the costs. We can base our decision on the grounds of legality, or on the grounds that one option appears more politically feasible than the rest. We can decide on the basis of majority will or fundamental fairness or because of self-interest, organizational interest or the interest of some group whose cause we are trying to advance. We can decide on instrumental grounds, that this policy would rectify a balance of payments disequilibrium or provide a consistent, low-cost energy supply. Failing all else, we can make a decision because heads came up rather than tails.

All of these are potential criteria for choice, and all identify standards for public judgment. One could incorporate any or all of them into a model of the decision-making process, and each, except perhaps for the last, has been highlighted somewhere in the policy-making literature as constituting the essence of decision. Nonetheless, the essential question is not how we can decide, or how we in fact do decide, but how we ought to decide. What counts as a good reason for a policy decision and what is an inappropriate basis for political judgment?

Any theory of policy evaluation[1] has to address the problem of the choice of criteria for decision making. This is as true for those who would propose models to explain the behavior of policy makers as it is for those whose interest is in trying to clarify what "policy rationality" or "good judgment" entails. However, it is probably fair to say that even purely "empirical" theories of policy process cannot escape the normative implications of policy evaluation. The move from the empirical to the prescriptive is easy and natural for the consumers of policy theory, and in the eyes of students and practitioners, current models of policy making readily become standards for the craft of policy analysis. Such has been the fate both of pluralist interest-group theory and incrementalist models of the budgetary process. (It is very easy for those who look upon political science as a repository of practical wisdom to conclude that it is "good practice" to inflate a budget request by approximately the amount of the anticipated legislative cut.) In any event, the "fact-value distinction" is not a profound stumbling block in policy theory. There are few enough students of policy analysis who are not concerned with the implications of different modes of policy choice and many significant works in the field—those of

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*I gratefully acknowledge the helpful comments of Murray Edelman, Booth Fowler, Leon Lindberg, Ben Page, Gina Sapiro, and John Witte on earlier drafts of this manuscript.

[1] Recently, the term "policy evaluation" has been appropriated to designate a sub-specialty within the general field of policy analysis, denoting the technical appraisal of the impact of public programs. I want to reserve the concept for a broader, more essential use, to refer to the process of making deliberate judgments on the worth of proposals for public action as well as on the success or failure of projects that have been put into effect.

Dror, Lindblom and Simon, for example—are overt arguments for preferred strategies of decision.

Logically, the choice of criteria is the first element in any theory of policy evaluation. How we perceive a problem depends on how we propose to evaluate it. Problems are not just "out there" waiting to be resolved. The first act of evaluation is to make a distinction between "problems" and "the way things are." Poverty is not a problem for a society that believes that "the poor are always with us"—or that they get precisely what they deserve. Inflation can be regarded as a "condition" rather than a "problem," for it means no more than a rise in the price structure. Inflation has to be regarded as a problem for a reason, as, for example, that it affects business confidence, or represents an illegitimate confiscation of some forms of property, or redistributes income in socially undesirable ways. A policy problem, in short, is a political condition that does not meet some standard.

It is not merely in the phase of problem identification that the choice of standards is important. Each step in the process of decision making depends on the initial stipulation of values to be served. We cannot just "weigh" or "compare" policy alternatives. We must weigh and compare them against something. At the end of analysis, we cannot merely make decisions. We also have to justify them. However whimsically or equivocally we *came* to our conclusions, good reasons have to be given for our policy preferences if they are to be taken seriously in the forums of policy deliberation. Ultimately, policy analysis has less to do with problem solving than with the process of argument. The better metaphor for policy analysis may not be the mathematical equation but the legal brief—it is a reasoned case for a preferred course of public action.

The Stipulation and Ranking of Values

Like the famous recipe for rabbit stew that begins, "Catch the rabbit," most formulas for rational policy choice begin with the admonition, "Identify and rank your values."[2] The problem for any theory of policy evaluation is how this should be done. Contemporary policy theory tends to side-step the issue. Either the selection of criteria is treated in a highly formal and abstract manner or policy rationality is regarded as a kind of economizing activity and a utilitarian conception of political judgment is implicitly or explicitly endorsed. A third alternative is to rely on a "political" conception of evaluation, in which feasibility or social agreement become overriding norms. In any event, in most contemporary formulations of the problem, the stipulation and ranking of values is regarded as more or less arbitrary, as a "given" in the appraisal of policy. The specification of values is not itself part of the process of rational choice. Rationality is defined in a purely instrumental sense, as "goal-directed behavior" (Riker and Ordeshook, 1973, p. 10). As Dahl and Lindblom write (1953, p. 38), "An action is rational to the extent that it is 'correctly' designed to maximize goal achievement, given the goal in question and the real world as it exists."

What virtually all contemporary policy theories have in common is a positivist or "emotivist" theory of evaluation. Values cannot be justified in terms of objective criteria. Hence, they must be regarded as "preferences" on the part of the policy maker. "Technical" or "rational" policy analysis can only begin once relevant values have been stipulated, either by an authoritative decision maker or through the statement of citizen preferences in a democratic political process. It is impossible to specify what standards ought to be taken into account in rendering a policy evaluation.

This, of course, is not the case. To be regarded as "reasonable," a policy recommendation must be justified as lawful; it must be plausibly argued that it is equitable and that it entails an efficient use of resources. Else it is subject to legitimate criticism.

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[2] Charles E. Lindblom's formulation (1968, p. 13) of the idea of policy rationality will do for an example:

1. Faced with a given problem,
2. A rational man first clarifies his goals, values, or objectives, and then ranks or otherwise organizes them in his mind;
3. He then lists all important possible ways of—policies for—achieving his goals
4. And investigates all the important consequences that would follow from each of his alternative policies,
5. At which point he is in a position to compare consequences of each policy with his goals
6. And so choose the policy with consequences most closely matching his goals.

For similar formulations, see Dror, 1968, pp. 129–49; Downs, 1957, p. 6, and Zeckhauser and Schaefer, 1968, p. 29. Pragmatic and incrementalist theories differ only in that standards are to be identified and ordered after the fact, in the light of the consequences of policy. See Brecht, 1959, p. 192.

It does appear that there are certain fundamental considerations that must be accounted for in any policy evaluation, a set of problems that can be identified with the classic concerns of political theory and with a repertoire of basic concepts including authority, the public interest, rights, justice, equality and efficiency. As standards of policy evaluation, these are not simply preferences. They are, in some sense, *obligatory* criteria of political judgment. To justify any policy recommendation, one must argue that it is within the legitimate powers of government, that it is, in some sense, "in the public interest," that it is consistent with lawful rights, that it is fair, and efficient in the use of resources.

The view that "comprehensive" policy rationality is impossible, as argued by Braybrooke and Lindblom (1963), March and Simon (1958) and others, then might not be precisely correct. While there may be an "infinite number of evaluations" for any policy question (as Lindblom argues in criticism of the classic rational model), it may be the case that there are only a limited number of values appropriate to address in rendering a policy evaluation. All policy argument leads back, eventually, to a finite and bounded set of classic principles and problems of political evaluation.

Furthermore, it may be that instrumental rationality is not the only conception of rationality pertinent to policy theory. The sense of rationality as "goal-directed behavior" is perfectly formal; it applies to *any* type of decision and not in any specific sense to *policy* decisions. It is an ideal of formal, logical validity that a given conclusion can be shown to derive from a given set of premises. However, formal validity is not the only tenable meaning of rationality in modern theories of logic. In the spirit of Wittgenstein's *Philosophical Investigations,* modern logicians like Stephen Toulmin (1969, p. 188) have argued that the problem of rational assessment—of telling sound arguments from untrustworthy ones—depends on the characteristics of an arena of discourse and the standards that are pertinent to it; or they have argued with Friedrich Waismann (1951, p. 117) that the known relations of logic can only hold between statements which belong to a homogeneous field of discourse. Thus, it may be possible to say more about *policy* rationality than about rational choice in general, and it may be that certain standards of judgment are necessary to rational policy choice because of the very nature of what we do when we make, and appraise, public decisions.

In the development of contemporary policy theory, what happened was that the ideal of the utility-maximizing individual was simply taken from positive economic theory and applied to the role of the policy maker. Policy rationality then becomes efficiency in the pursuit of any set of utilities. However influential this instrumental conception of rationality may have been in defining the dominant models of contemporary policy analysis, it is by no means the only acceptable use of the term. In modern analytic philosophy, it is also possible to conceive of rationality as performing an action for reasons that an individual correctly or reasonably regards as good reasons (Benn and Mortimore, 1976, p. 2). This conception of rationality as acting for good reasons leads to two related senses in which an action can be called rational. Either we ascribe rationality to individuals who act for reasons which they regard as good, or we call actions rational when we judge that there are reasons to support them, as when we say, "That seemed a rational thing to do." This conception of rationality seems particularly germane to the process of policy discourse and policy argument. And when we consider what would count as a good reason in policy discourse, it would seem that we are inevitably led back to the classic concerns of political evaluation and the classic principles of political thought.

However, merely to invoke the "classic principles" as criteria for policy evaluation is not to have done very much. To talk about "justifying" decisions in the light of such principles seems to lead us down a familiar and none-too-promising path. We begin to ask questions like "What is justice?" and "What is the public interest?" seeking an essentialist definition that would serve as a conclusive test of the propriety or impropriety of public action. It is not my intention to propose an idealist theory of policy evaluation. I want to stay safely within the positivist and pragmatic tradition that has been dominant within this field of political theory, though perhaps to reveal some unexpected implications of what is possible within that tradition. To this end, we must be quite clear about the kind of problem involved in the selection of criteria for policy making and in defining the logical status of the classic principles in any theory of policy evaluation.

The Pluralism of Policy Languages

We can begin from a consideration of a practical problem of policy appraisal. If, as I have argued, standards of evaluation are embedded in specific fields of discourse, if they arise from specific paradigms of inquiry and analysis, then our sense for what counts as a good reason and as a mistake depends on the ideological, disciplinary, or cultural context within which we are operating. This is a familiar problem, both of the sociology of knowledge and of the philosophy of science, that our "plausibility structures" have much to do with social context and social reinforcement. However, the crucial difficulty is not merely the relativity and contingency of standards that such a view entails. The significant issue is how we should proceed when we are aware that public issues can be perceived and appraised through multiple frameworks of evaluation. Given the diversity of the policy sciences in our times, this has become a characteristic dilemma of policy choice and policy rationality.

The proliferation of "policy professions" in the past generation or so has created a great variety of languages and logics of policy evaluation and justification. In such a field as energy policy, for example, the policy maker or citizen must somehow decide what to make of the various arguments and analyses presented by economists, environmentalists, engineers, scientists, lawyers, and so on, each claiming to be authoritative for a specific aspect of the problem, each justified by its own premises of inquiry and rigorous logic of analysis, each containing imperatives for definitive public action.

In the conventional wisdom of policy making, policy makers specify objectives and policy professionals deal only with means to politically designated ends. However, the conventional wisdom may have the matter almost precisely backwards. The actual role of policy professionalism in contemporary government is probably more prescriptive than instrumental. The setting of standards of good practice is a large part of what professionalism means. Most policy professions are such precisely because they provide standards for public policy. In such diverse fields as forestry, public health, nutrition and welfare, the essential function of the expert is often that of setting criteria for the definition of public objectives and the appraisal of public programs.

Policy evaluation takes place within and between such languages of policy analysis. Like scientific disciplines, policy languages constitute a realm of discourse and of argument. What counts as a "problem," and a "good reason," and as a "mistake" in judgment depends on the normative standards embedded in a specific framework of analysis. The process of policy evaluation and argument is different in law, macro-economics, diplomacy, environmental planning and civil engineering. In each of these professions, different rules exist for the identification of the problematic; there are different norms of evaluation and different criteria for what would count as a "solution" to a public problem, for how we would distinguish a "successful" public venture from a policy "failure."

This pluralism of policy languages does not pose any ultimate problems of justification in itself. All arguments trail back eventually to certain normative postulates and assumptions widely shared in liberal political discourse (and arguably, by Marxian and Catholic corporatist thought as well). When we *justify* a policy standard (address the question of why it is appropriately invoked as definitive in making judgments about alternative public projects and purposes) we generally appeal to some higher-order principle with which the criterion of choice is deemed to be consistent (Taylor, 1961, pp. 45–46). Thus, the neoclassical economist, when asked why "market failure" is an appropriate (and perhaps exclusive) test of the propriety of government intervention in the economy, will refer back to the values of freedom and efficiency served by the market mechanism and behind this perhaps to some notion of the moral autonomy of the individual and to a nominalist skepticism toward the possibility of objective standards of public judgment apart from individual preference and wants. Similarly, if one inquired of the committed conservationist why it might not be a plausible energy policy alternative to simply use up all fossil fuels in one generation of grand bacchanalian extravagance and let the future fend for itself, appeal would probably be made, in the final analysis, to some notion of justice between generations (Barry, 1977).

It is not then that the diverse policy professions do not rest on a shared set of justificatory principles. Rather, the problem is that the weight, emphasis and significance given to the various fundamental principles is different in each. Because they endorse different standards of argument (what counts as a good reason) and because they are organized to account for different facets of experience, they entail different orderings of relevant values. Thus, the central normative construct of law is rights, while that of economics is efficiency. While it might be possible to argue considerations of efficiency in delimiting the scope of an established right (a court might plausibly argue potential bankruptcy as a ground for denying counsel to indigent defendants), in legal reasoning, the ordinary procedure for establishing the categories and scope of rights is not that of a calculus of individual utilities. Conversely, while the logic of economic reasoning may rest on certain postulates of rights (such as property rights), the purpose of a calculation of alternative utility functions is not to stipulate a system of rights.

Hence, it makes a great deal of difference whether we choose to adopt the rules of rational discourse of one of these policy professions rather than another in evaluating a policy problem. We may come to very different conclusions if we choose to regard a specific issue as falling in the domain of law rather than economics. As Arthur Okun (1975) suggests, it makes a difference whether we regard a problem like income distribution from the point of view of rights rather than that of efficiency. There is, he suggests, a "big tradeoff" between these two considerations, either of which can be regarded as definitive for policy making.

In such a pluralistic universe of policy discourse and policy rationality, the crucial problem for a theory of policy evaluation becomes that of identifying standards by which we may weigh the claims of various languages of policy analysis. On what grounds should we choose to regard a problem from the point of view of one of the policy professions rather than another? What justifies the choice among the several logics of policy evaluation?

This is to put the "metapolitical" problem in applied form. Consider the following situation. A labor leader is deciding whether to declare an illegal strike. If the decision is made to strike on the grounds that the probable benefits would be greater than the probable costs, we would say that the decision was based on economic reasoning. If the strike is not called because of its illegality, we would say that the decision was based on legal grounds. But what guidance can be given as to whether the leader ought to decide for economic or legal reasons?

Hence, the problem that must be faced in constructing a theory of policy evaluation is also a characteristic practical problem of policy making in our times. How do we judge a judgment? What standards must any system of policy evaluation meet? The problem concerns the justification of the procedures of policy analysis, but it also addresses the concrete question of how we weigh the rival claims and considerations adduced by different "rationalities" of policy appraisal.

The Metapolitics of Policy Evaluation

A few political scientists have begun the process of trying to extend the idea of policy rationality, asking what is logically required in any stipulation of criteria for policy evaluation. For example, Brian Barry and Douglas Rae (1975, pp. 337–401) set forth seven requirements that any system of political evaluation must meet. Their standards derive largely from formal decision theory. They are rules that must be followed if we are to judge rationally among alternatives in the light of standards.

For Barry and Rae, the formal tests of adequacy are these. The standards by which a policy is judged must be *internally consistent*, that is, transitive. It is logically impossible to come to conclusions about preferred policy unless the criteria are clear and hierarchically ranked. Standards must be *interpretable:* we must be able to tell what a standard means in policy discourse, to judge when it has been met or fails to be met. It must be possible to *aggregate criteria,* to rank them, for a variety of standards might be relevant to choice; yet we cannot simply say, for example, that we favor both, say, equity and efficiency, for no alternative may satisfy both criteria equally.

Any theory of policy evaluation, for Barry and Rae, must also account for the problem of forced choice, that not to decide is in fact a policy choice, and there is no a priori reason why the status quo should have a privileged position in policy appraisal. A system of political evaluation must acknowledge *risk and uncertainty* (we cannot say with confidence, that a given choice will actually meet standards) and it must deal with *time* (policies will have both proximate and distant consequences). Finally, choices must be logically justified in terms of some conception of *individual welfare.*[3]

Duncan MacRae (1976) has attempted to develop a "metaethics" for normative policy discourse on the basis of an analogy between the logic of policy evaluation and the rules of scientific inquiry. MacRae suggests that normative discourse can be as rigorous as empirical investigation in politics, that it is not mere assertion, but can be seen as disciplined inquiry to be judged by "higher-order" standards of appropriate procedure. MacRae envisions normative discourse as debate between the proponents of rival evaluative systems. The standards that any system must meet, the grounds of appropriate criticism of any normative judgment, include (1) *generality*, if the proposed system fails to apply to a choice about which both discussants have moral convictions and to which the critic's system does apply; (2) *internal consistency*, in which the proponent's system yields contradictory recommendations where the critic's does not; (3) *inconsistency with shared convictions*, that the proposed system leads to conclusions at odds with presumably shared evaluations.

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[3] Barry and Rae also take up a rigorous critique of certain specific criteria of decision, including utilitarianism, equality, Pareto optimality, majority, minimax and dominance and they do consider the place of principles in political evaluation, about which more further on.

I offer no critique of the position of either Barry and Rae or MacRae. Both have extended and clarified the domain of rationality in policy evaluation. Yet, I think the logic of their analysis can be carried further. They emphasize procedural norms, formal rules of consistency and logical rules of validity in rational choice and ethical judgment. However, it seems possible to continue this line of logic to assimilate certain substantive principles that distinguish policy judgment from the more general language regions of rational choice or normative discourse. Barry and Rae, to be sure, do take up the place of the classic principles in policy analysis. However, they view such concepts as justice, rights, the public interest, etc., primarily as "simplifying devices," culturally derived constructs that somehow pull together and focus attention on certain facets of our shared political experiences. The logical status of such principles is somehow different from that of the formal norms of rational decision making. My own view is that something more can be said for and about the place of such principles as "metapolitical" standards, as criteria for judging the worth of any system of policy evaluation.

The Place of Principles in Policy Analysis

It is not immediately apparent that any systematic relationship exists among the classic principles of political thought. Words like *right, the public interest, justice, freedom* and *community* can seem to be no more than an array of discrete "good things." We are used to the idea that political argument can fasten arbitrarily on one or a few of these concepts and that they can be arranged in different patterns in ideological thinking, invested with a variety of meanings and given different degrees of emphasis.

To make the point that the criteria which enter into a policy evaluation are not merely preferences, but that certain substantive standards need be accounted for in any rationally defensible policy judgment, one must show that certain principles do form, in some sense, a finite and bounded set and one must establish in what specific way such criteria are mandatory in the activity of policy evaluation. It is also necessary to show that these principles can be stated in such a way as to be free from any partisan or ideological quality. They must reflect no more than the logical characteristics of any form of political judgment.

The essence of my argument is this: a defensible policy judgment must meet certain formal standards of rational choice—it must have regard for such problems as the transitivity of values and forced choice—but it must have other qualities as well. By virtue of the fact that these are policy judgments, certain political values must be taken into account. It is not that these values can be justified in any ultimate sense. I will not argue the Kantian position that there are certain normative propositions that any rational thinker must endorse to remain consistent, nor will I argue, with Rawls, that there are certain principles that self-interested individuals would be led to accept if placed in the "original position" and called upon to formulate basic norms of political life. Rather, the argument is that certain criteria of choice are inherent in the activity of politics itself, that they are part of what we mean by "making a political judgment" or as Wittgenstein might have put it, that they are part of politics as a "form of life."

My case is that a specific set of principles is *obligatory* in political evaluation in a very special sense. It is important to be very clear about the logical status of such principles as criteria of "metapolitics"—standards for judging the adequacy of any system of political judgment. There is a difference between claiming that certain *concepts* are indispensable in political evaluation and that certain *propositions* are. To say that a comprehensive policy analysis must account for the problem of authority is not to say that a binding policy recommendation must be shown to "derive from the will of the people." As Stephen Toulmin says (1972, p. 418),

> it is one thing to argue that a certain concept is something that a rational thinker or agent cannot very well get along without; it is quite another to establish the necessary truth of certain substantive ethical or scientific principles.

As standards at the "second level," as criteria for evaluating a policy evaluation itself, the classic principles have a particular standing. They are not norms of conduct; rather, they identify those aspects of a policy recommendation that require justification. The status of such principles in normative policy theory is not unlike the standing of a hypothesis in scientific inquiry. As a hypothesis *requires* experimentation to test its validity, so "principles," in the sense I shall be using the term, require justification to demonstrate the rational plausibility of a policy recommendation.

Taken in this sense, it is possible to identify three of the classic principles that appear to be rationally requisite to the construction of a defensible policy evaluation. These are authority, justice and efficiency. The relationship of these to other fundamental political ideas can also be specified. Nonetheless, this statement is to be taken as exploratory and provisional. While I think it is possible to demonstrate that these considerations, *at least*, have to be accounted for in the process of policy evaluation, it is possible that a similar case can be made for other principles as well.

Authority. In the most general sense, authority means that an exercise of power is rightful, which is to say, justifiable. This implies that good reasons can be given for the act of policy making, that an entitlement can be established to decide on the public behalf and a concomitant obligation to abide by such decisions. Thus understood, authority is a necessary characteristic of any legitimate policy decision and a requirement of any system of policy evaluation. A decision which cannot be justified in terms of this standard is simply an act of domination or coercion.

Put another way, as a logical characteristic of any system of policy evaluation, authority functions to structure the terms of the public debate. It locates the burden of proof in political argument. The logic of public policy making requires that the burden of proof rest always with the state (Neuman, 1953, p. 904). It is up to government to justify its actions; it is not up to citizens to demonstrate why policy makers should not do precisely as they please. It is hard to imagine how the presumption could be reversed and retain any meaningful sense of political discourse. If we were to accept that public action might just as well be whimsical and arbitrary, no problem of political evaluation really arises.

To justify a policy evaluation, then, reasons have to be given for regarding a problem as properly resolved through public action. In liberal political thought, the problem of authority is systematically related to such principles as individual freedom and consent. In liberal discourse, the problem of justification takes the form of giving good reasons for overriding the presumption in favor of individual autonomy. Any legitimate public action must be shown to derive, in some sense, from consent or individual wants and interests. However, the problem is not at all peculiar to liberalism. "Divine right" was, after all, a formally rationalized justification for the exercise of authority. Sixteenth- and seventeenth-century doctrines of sovereignty were an attempt to "give reasons" for unlimited authority. Marxism is an elaborate critique of the consistency of the liberal notion of authority and provides an alternative system of criteria for regarding public action as appropriate or inappropriate. (In fact, Marx, like Rousseau, can be regarded as doing no more than making the liberal presumption in favor of freedom a more stringent test of the legitimacy of public action.)

Even the so-called "irrationalist" political theories, those that *argue* that rationality is far from a definitive force in human affairs, rest paradoxically on a reasoned justification for a certain pattern of authority. Thus, A. James Gregor writes (1969, p. 19):

> Adolf Hitler's *Mein Kampf* can, in no sense, be conceived as *simply* a statement of the author's personal emotional preferences. Suggestions, admonitions and imperatives are *argued.* The arguments may be elliptical–vital premises may be suppressed–or they may be invalid and their factual premises erroneous, but they are not simply assertions of personal preference. Hitler advocates, for example, the fulfillment of one of mankind's highest aspirations; man's continued evolutionary progress. . . . We raise objections when Hitler contends that the fulfillment of such an ideal demands the caste superordination of a specific biological race. . . . We demand evidence, for example, that would confirm that a given race is the sole repository of man's foremost creative capacities. But such objections raise questions of fact and definition and as such are subject to cognitive appraisal.

In any rational policy evaluation, then, good reasons have to be given for regarding a problem or project as appropriately the subject of public action. In the contemporary languages of the policy professions, this can be and is done in a variety of ways. The neoclassical political economist may argue that "market failure" or bona fide "public goods" are the only grounds for state intervention that legitimately override the presumption in favor of autonomous, individual choice. The utilitarian may appeal to some aggregate social welfare function as an appropriate test that "authorizes" a public action. The social democrat may argue from some need or desert-regarding conception of distributive justice. The legal positivist may simply hold that a policy option is legitimate if it is apt to be upheld by the courts, while the constitutionalist will admit the propriety of considering a policy option if it squares with the formal powers of government. Whatever the particular reasons offered, the essential point is that *some* justification on the grounds of authority has to be provided. Authority is not simply a "preference" of the policy maker that may be introduced as a criterion for the evaluation of public policy or not as one likes. One simply cannot say, in justifying a policy appraisal, "I don't care if it's blatantly illegal and rides roughshod over established rights; it's still the most efficient way of getting the job done."

There is an intimate connection between the principle of authority, thus understood, and such concepts as freedom, rights and justice. These are grounds of argument, criteria that can be introduced to make claims about authority, or to test propositions about authority. Different conceptions of freedom—the distinction between the "freedom from" imperatives of the classic liberal and the more activist sense of "freedom to" (Berlin, 1969, pp. 118–72)—give rise to different images of legitimate authority. It might be argued that freedom is as much an obligatory consideration in any political judgment as is the concept of authority itself. (Certainly, the very notion of political argument implies a notion of the individual capable of judging the worth of rival claims.) Yet, it is such only in relation to the problem of authority, or justice. We make propositions about human freedom in order to arrive at a conception of legitimate authority. We do not make cases about authority so as to arrive at a conception of human freedom.

The concept of "the public interest" also bears a close relationship to the problem of authority. It is easy enough to demonstrate that there is no definition of the public interest to which all reasonable persons would necessarily repair. It is equally easy to show that any rational policy evaluation must give reasons for regarding a policy proposal as, in some sense, in the public interest. One cannot justify a policy recommendation on the grounds that "it would make me and my friends richer." However refreshing the candor of such an argument might be, it does not and cannot stand as a legitimate warrant for a public action. (When self-interest is argued as a legitimate basis for decision it is always in connection with some theory of how the public interest will arise from the competition or aggregation of individual interests.)

The concept of the public interest is itself logically dependent on other fundamental criteria of political evaluation, among them concepts of individual welfare and community. The connection between individual and collective welfare must somehow be accounted for in any comprehensive policy evaluation and it is necessary to specify precisely which "public's" interest is to be taken into account. Any policy appraisal entails a decision about who is properly regarded as "us" and "the others," whose interests are to be promoted and whose thwarted or ignored. It makes a considerable difference if the relevant community in whose "interest" policy is to be made excludes the adjoining class, county, nation, the rest of humanity or the people who will live five centuries hence.

Thus, a rigorous and comprehensive policy analysis must touch a number of bases to establish the propriety of public action. This does not mean that every policy argument must contain a full-fledged case about authority going back to first principles and premises. To think of policy analysis and debate in this way would be tedious indeed. Authority may not be problematic in all exercises in policy evaluation. It may simply be assumed that the available array of alternatives passes the test of legitimacy. There are short cuts and simplifying devices in the ordinary language of any policy debate. What is certain, however, is that if a policy is controversial, it will be so in relation to one of the fundamental dimensions of evaluation. Implicitly or explicitly, a policy recommendation must be regarded as plausible in terms of each of these considerations.

Justice. John Rawls (1971, p. 3) states that "justice is the first virtue of social institutions, as truth is of systems of thought." It may or may not be the case that justice must rank first in any ordering of criteria for policy evaluation. Justice might be taken as the paramount criterion for policy evaluation, as a test, say, of rival conceptions of authority. This is the thrust of Marxian analysis and modern theories of civil disobedience, which suggest that a system of government is not rightful if it can be shown that prevailing standards of authority do not permit the consideration of the more equitable policy alternatives. On the other hand, one can argue, with Jimmy Carter, that "the world is unfair," that government should deal only with those cases of inequity that meet the tests of authority and efficiency (and therefore, that welfare funds need not be made available to pay for abortions). It does not seem possible to specify a *necessary* priority between the principles of authority and justice. However, this does not mean that the ranking of these standards of policy evaluation is simply a matter of the policy maker's preference. Good reasons must be given to justify assigning priority to one or the other of these values. Furthermore, it would seem possible to show that justice is not an optional but a necessary consideration in any system of policy evaluation.

This is the case not only because any public decision will have distributional consequences but also because it is inherent in the logic of policy making that rules must either be stated universally or so as to apply to particular cases and categories. The maxim of justice, "Treat like cases alike and different cases differently," stands then not as an ethical imperative but as a statement of a problem that requires justification: it raises the question of what counts as a good reason for regarding cases as alike or dissimilar. As Benn and Peters (1959, p. 99) put it: "If, for example, we believe in democratic rights for white men, we must show good grounds for denying them to black, and the simple criterion of skin color alone is not an obviously relevant ground of distinction. There may be other and better grounds—but the onus of proof rests on those who would limit the right, not on those who would give it universal scope."[2]

The concept of equality then is probably best regarded not as an independent consideration in policy evaluation, but as a proposition that stands in a logical relationship to the problem of justice (Perelman, 1963, pp. 1–60; Bedau, 1971; Benn and Peters, 1959, pp. 107–34). In liberal argument, equality locates the burden of proof in making cases about justice, as freedom does in relation to the problem of authority. All individuals are to be regarded as in the same position with regard to policy unless good reasons can be given for treating them differently. Grounds for differentiation may include need or desert, merit, compensation, or some public interest criterion (as in the familiar appeal to the need for incentives as a spur to productivity and economic growth, which stands as a proxy for aggregate social utility in most modern economic analysis). To my knowledge, there has never been a pure egalitarian argument that cases are to be treated alike regardless of circumstances. Most contemporary egalitarian arguments rest on the premise that existing inequalities are unjust, and that failing a reasonable justification for differential treatment, cases should be treated alike. Thus it seems appropriate to regard justice and not equality as the necessary consideration in any system of policy evaluation.

The point then is that any policy evaluation must include a justification of the categories of universal or differential treatment to be established (though liberals might argue that equality requires no justification) (Feinberg, 1973, pp. 98–102). Once again, it does not make sense to regard justice simply as a preference of policy makers, a consideration which they are free to incorporate into a policy evaluation or not as they wish. One cannot really vindicate a policy recommendation by asserting, "I know this program is unfair, but it is efficient, and we clearly have the authority to enact it."

Efficiency. It is easy enough to show that efficiency is a necessary consideration in any system of policy evaluation. Means must be appropriate to the ends chosen, and it is a legitimate criticism of any policy recommendation to demonstrate that there are better alternatives for achieving stipulated values. What is more important is to demonstrate that efficiency cannot be the exclusive consideration in rendering a policy appraisal.

In many models of policy evaluation, efficiency is regarded as tantamount to rationality. The problem of policy is "solved" when it can be demonstrated that one alternative yields an optimum level of benefits over costs. As we have noted, rationality is often defined in an instrumental sense, as "goal-directed behavior" in policy theory. Yet, some conceptions of policy rationality carry the matter even further. They argue that the efficient solution to a policy problem is also the solution that is necessarily in the public interest. In fact, latent in the logic of much of contemporary political economy, whether founded on neoclassical or utilitarian premises, is the notion that to solve for the problem of efficiency is simultaneously to solve for the problems of authority and of justice.

The cost-benefit analysis is a fundamental paradigm of much contemporary policy analysis. In the most general sense, any cost-benefit analysis rests on a utilitarian foundation. The only appropriate (or possible) criterion of policy is individual wants and interests, somehow aggregated to yield a social utility function. Any policy which reduces social costs or increases net social welfare is "beneficial." The standard of policy evaluation may be cast in terms of Paretc optimality: a desirable policy is one which makes someone better off without making anyone worse off (or in terms of the looser formulation, a desirable policy is one where winners could conceivably compensate losers through costless transfers). In either case, a solution to the problem of efficiency also yields a solution to the problem of the public interest.

Thus, is can be argued, as it has been, that cost-benefit analysis can be justified as a mode of policy evaluation, that it accounts for those considerations that are necessarily incorporated in any rational system of policy appraisal. However, this is not a necessary conclusion, and in fact most economists do not understand the force of cost-benefit analysis in this way. Efficiency is better identified as one element, but not the exclusive element in policy discourse. As E. J. Mishan says (1973, p. 13),

> It cannot be too strongly stressed, however, that even the result of an ideally conducted cost-benefit analysis does not, of itself, constitute a prescription for society. Since it simulates the effects of an ideal price-system, the ideal cost-benefit analysis is also subject to its limitations. Any adopted criterion of a cost-benefit analysis, that is, requires *inter alia* that all benefits exceed costs, and therefore can be vindicated by a social judgment that an economic rearrangement that *can* make everyone better off is an economic improvement. The reader's attention is drawn to the fact that such a judgment does not require that everyone is actually made better off, or even that nobody is actually worse off. The likelihood . . . that some people, occasionally most people, will be worse off . . . is tacitly acknowledged. A project that is adjudged feasible by reference to a cost-benefit analysis is, therefore, consistent with an economic arrangement that makes the rich richer and the poor poorer. It is also consistent with manifest inequity, for an enterprise that is an attractive proposition by the lights of a cost-benefit calculation may be one that offers opportunities for greater profits and pleasure by one group, in the pursuit of which substantial damages and suffering may be endured by other groups.
>
> In order, then, for a mooted enterprise to be socially approved, it is not enough that the outcome of an ideal cost-benefit analysis is positive. It must also be shown that the resulting distributional changes are not regressive, and no gross inequities are perpetrated.

Even an ideal cost-benefit analysis, then, is normatively incomplete; it cannot escape criticism on the grounds that are pertinent to any policy appraisal. However, this is not the only difficulty with the kind of utilitarian calculation that cost-benefit analysis represents. Since it is impossible in practice to know and to aggregate individual utilities, any concrete exercise in cost-benefit analysis requires a stipulation of the costs and benefits to be taken into account. The values postulated, and their ranking, has to be defended on *some* basis. Alasdair MacIntyre (1977, p. 226) points out that "the use of a cost-benefit analysis clearly presupposes a prior decision as to what is a cost and what a benefit" and that this decision has to rest on some ground other than utilitarianism itself.

It is not possible to derive a complete policy evaluation from any form of purely economic analysis. Efficiency is properly regarded as one ground of policy evaluation, a necessary one to be sure, but not as the exclusive consideration in policy appraisal. Efficiency is best regarded as an instrumental value, a tool for comparing policy options in terms of other values. In fact, in any cost-benefit analysis, we do not compare all alternatives but only those that have survived scrutiny on other evaluative grounds. In making an economic analysis of different road-building techniques, we do not draw up a neat comparison of the costs and benefits of *corvee* and slave labor in relation to more conventional methods. In terms of our standards of authority and justice, these options are simply out of bounds.

If efficiency is properly regarded in this instrumental sense, then it is a lower-order criterion of political judgment, basically a "tie-breaker" between policy options that have passed minimum tests of acceptability on grounds of authority and justice. If this is in fact the case, then it is possible to specify not only the values that have to be taken into account in any comprehensive policy evaluation but to advance certain propositions about their ranking as well. Though it is difficult to stipulate a logical priority between authority and justice (some would argue that justice is the supreme test of the propriety of any exercise of public authority, others that government should only act with regard to those forms of injustice that fall within its legitimate powers), it is possible to say that efficiency can never be assigned a higher priority than authority or justice in any plausible value ordering. Again, we simply cannot argue, "I realize that this policy is unjust; nonetheless, it is the most efficient way of achieving our objectives."

The propositions I have outlined would seem to raise problems of democratic theory. If individual wants, somehow aggregated into "public wants," are not to be regarded as a definitive test of policy, then neither, it would seem, is "majority will." To be sure, in any democratic polity majority will, expressed through prescribed constitutional mechanisms, is accepted as the definitive test of authority. A policy is legitimate insofar as it reflects the "will of the people." However, unless we are very dogmatic "absolute majoritarians" we do recognize that to regard majority will as the exclusive standard for the evaluation of policy is both paradoxical and unsatisfactory. Within most versions of democratic theory, it is an appropriate criticism of majority will to argue that what the majority wants is unjust (as, for example, if the majority wills genocide against a minority) or inefficient (if the policy preferred by the majority would lead to national bankruptcy). The puzzles and quandaries that arise in relation to any simplistic conception of democratic theory suggest that the majoritarian principle cannot be the exclusive ground of justification. In fact, of course, within any democratic polity, we do judge the worth of majoritarian policies on other grounds.

Conclusion

The problem for a theory of policy evaluation is not to discover the right policies but to establish the appropriate grounds for decision making. Not all reasons are good reasons for a policy decision. One cannot justify a public decision simply by invoking a schedule of personal preferences. A justifiable policy evaluation must meet certain standards; it must address specific problems that are characteristic of the activity and enterprise of politics itself.

To say that a policy is justifiable is not the same as saying that one will endorse it. Rather, it means that we regard the basis on which it was made to be plausible, or rationally defensible. A plausible policy argument is one worth taking seriously in the public debate. An impartial observer would weigh its claims and a critic would feel obliged to respond with careful counter-argument.

There are in fact only a finite number of kinds of reasons that can and must be given in justification of a policy recommendation, a logically delimited set of grounds that are appropriate to the appraisal of public policy. It is possible to set standards of comprehensiveness in policy evaluation, to identify those questions that must at least be addressed if a policy appraisal is to appear "worthy of consideration" in our eyes.

It is not simply the stipulation of values to be served that requires reasoned justification but their ordering as well. One cannot state a transitive ranking of values simply as a matter of preference. If efficiency is to be regarded as more than an instrumental value, reasons will have to be given for regarding the most efficient solution as also within the range of "just" alternatives, and as in some sense "in the public interest."

It might be argued that this conception of policy rationality does not transcend our ordinary sense for what is at issue when we face dilemmas of public choice or when we criticize prevailing notions of how public issues ought to be perceived or analyzed. I admit that my discussion of the limitations of instrumental rationality is hardly novel and my conception of the critical political principles unexceptional. And that is precisely the point. There is something wrong when our dominant models of policy rationality are so obviously at odds with what we in fact take to be fundamental considerations in the deliberation of policy choices. My object has simply been to make explicit what we do regard as legitimate grounds of criticism of any framework for policy choice, and to suggest that our "ordinary language" is probably not deceptive, that certain standards are inherent in the logic of policy evaluation itself.

It can also be argued that this analysis has not "solved" the problem of policy choice, that it provides no prescriptive tests which the practitioner might apply in making decisions about desirable and undesirable public purposes or options for action. That, of course, was not in the cards. In order to reduce policy analysis to a routine, rather than an object of deliberation, one must state criteria of judgment in the form of rules. As Barry and Rae suggest, it is a requirement of any "first-order" system of evaluation that standards be interpretable, that we be able to gauge clearly when a policy meets the mark or fails to do so. The sense of policy rationality described here does not pertain to decisions of this type. Rather, it comes into play at the second, or "metapolitical" level, when we are trying to decide how to decide, when we must face the decision of which system of standards to apply to a given situation.

We face a genuine dilemma of decision only when we are aware that public purposes can be perceived and appraised in more than one way. As I noted, given the proliferation of policy languages in our time, we increasingly face quandaries of this type. So long as we are operating within a bounded framework of policy evaluation, we face no such perplexity. We can "justify" first-order decision rules according to the higher-order value propositions with which they are deemed to be consistent. It is when we understand that different frameworks of evaluation could be applied to a given situation of policy choice that we face a decision of a different kind. The standards that apply to such a judgment have a different logical character from those that apply to first-order policy choices. We must invoke criteria that test the adequacy of any system of policy evaluation. The requirements of formal decision theory, as described by Barry and Rae, pertain to decisions of this type. So do the rules of normative consistency proposed by Duncan MacRae. My point has been that a certain conception of the classic political principles, understood as necessary considerations rather than as explicit normative propositions, is essential to complete this expanded sense of policy rationality.

The classic principles, then, have a place in policy theory that is very much like that of the norms of inquiry in scientific investigation. Thus, the sense of the concept "validity" in scientific discourse is not unlike that of "justification" in policy evaluation. Validity has no clear, incontestable meaning as a standard of science and different disciplines endorse different rules concerning what counts as a valid scientific argument and what does not. By the same token, different policy languages and professions endorse different notions of what counts as a justifiable policy proposal.

One basic norm of scientific inquiry is that propositions be stated in "operational" form, that one can at least imagine an objective procedure that would identify the presence or absence of a phenomenon. Again, different scientific disciplines endorse different standards regarding the kind of scientific evidence that is appropriate or obligatory, what kinds of reasons can or must be given to establish the plausibility of a scientific hypothesis. In the same way, the maxim of justice that "like cases be treated alike and different cases differently" establishes no necessary test of the justice of a public project. Different policy languages endorse different standards regarding the equity considerations that would establish the plausibility of a public proposal. Yet just as *any* scientific argument must account for the problem of experimental demonstration, so also *any* given policy argument must account for the problem of the propriety of universal or differential treatment of cases.

There is no final justification for the norms of scientific inquiry or for those of policy evaluation. There is no way of showing that a policy *must* be equitable any more than it can be shown that a proposition *must* be verifiable. What can be said instead is that just as there is nothing binding about the norms of scientific inquiry unless we are doing science, there is also nothing binding about the norms of political discourse unless we are engaged in policy evaluation.

Inevitably, the policy sciences will endorse some conception of how one ought to proceed when faced with a problem of public choice. The function of policy theory is not merely to explain why we get the kind of public policies that we do, but to provide guidance as to what is involved in thinking through a public problem to a reasonable conclusion. The very notion of policy evaluation implies that we will make judgments about desirable and undesirable public purposes and projects on some grounds.

The models of instrumental rationality that have provided our dominant strategies of decision do not adequately represent what we acknowledge to be at issue when we confront public decisions. Models of decision which place the stipulation and ordering of the values which will serve as objectives and constraints on choice outside the realm of policy rationality provide a truncated and unsatisfactory foundation for policy theory. They place the policy analyst in the position of an agent to an authoritative policy maker. The mission of policy analysis becomes that of maximizing the evaluative preferences of a particular client. Either that, or one invokes some equally problematic norm of "public responsiveness" which similarly evades the fact that in policy making we do debate the grounds on which public issues ought to be decided. However, it has generally been assumed that no other moves were possible without introducing some absolute criteria of decision, which would violate the fundamental norms of scientific neutrality and rational generality on which any comprehensive theory for the policy sciences would have to rest.

This analysis suggests that it is possible to treat the question of specifying the substantive standards that are properly introduced in any comprehensive policy evaluation as well as the rules that appropriately guide choice for any stipulated set of values along lines that are perfectly compatible with the fundamental norms of modern policy theory. In doing so, it opens up the possibility of an alternative model of policy rationality, one in which policy making is understood as a process of reasoned deliberation, argument and criticism rather than a pragmatic calculus. However, this more comprehensive vision of policy choice does not displace but rather complements the formal requirements of instrumental rationality. Furthermore, it does not imply that institutional and sociological factors are unimportant elements in the calculus of choice. Rather, it is simply explicitly recognized that to follow dictates of situational or strategic necessity is not a sufficient criterion of good political judgment. Finally, by suggesting that normative political argument has its own strenuous rules and standards, one defines a rather precise relationship between the perennial concerns of political philosophy and the paradigms of practical reason which the policy sciences aspire to develop.

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The Parties Come to Order! Dimensions of Preferential Choice in the West German Electorate, 1961-1976*

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Spatial analyses of party systems typically construe the dimensions of party configurations in terms of policy-defined issues and ideologies such as the left-right framework. But as far as the mass public is concerned, perceptions and evaluations of parties in those terms require a degree of political sophistication beyond the grasp of the average voter. That is why this article explores the impact of alternative criteria. Its main thesis is that in multiparty systems with coalition governments the party alignments in government–and possibly in opposition–provide a cue to the public of great consequence for its preferential choice. Party preference orders of the West German electorate are examined by way of unfolding analysis. The data come from a series of surveys collected during the period 1961–1976. The results of the analysis reveal traces of the two dominant cleavages of German politics, namely social class and religion, but fail to show any significant influence of the left-right framework. What is most conspicuous is the susceptibility of preferential choice to cues emanating from the coalition behavior of party leaders. Given a context of changing coalition alignments among parties in government, that susceptibility prevents any fixed configuration of parties from governing voters' preferential choice.

Few areas of political inquiry have witnessed such a rapid flowering in recent years as the area of spatial analysis of party systems. The stimulus of Downs' (1957)[1] work combined with the spread of multidimensional scaling techniques has spawned a rich body of configurational analyses, depicting the location of parties, groups, issues, candidates and, sometimes, voters in joint geometric spaces (e.g., Weisberg and Rusk, 1970, and Budge et al., 1976). Such analyses would seem especially useful for the analysis of party distances and cleavage dimensions in multiparty systems. They provide a map of party locations in the political space and, using that map, we might be able to explain both the coalition building among parties in government and electoral choice among voters.

The Downsean theory, like most spatial analysis, assumes that the dimensions defining the political space are ideological in nature, with the left-right continuum being the most prominent dimension; at least such analyses refer to policy issues of high and lasting significance. Voters are supposed to perceive parties (and candidates) as located along those policy-defined dimensions and to select whatever party (or candidate) is least distant from their own locations. Stokes (1966) has questioned the key premises of this notion, and Converse (1966) has called attention to the intricacies of the "party distance" notion. Few, if any, spatial analyses have actually tried to test how well *individual* preferential choices fit the "ideological distance" model and none has demonstrated such a fit. The typical procedures used in multidimensional analysis are not suitable for such a test, anyway.

It is the goal of this article to explore the contribution of nonideological criteria to voters' preferential choice among parties. As Stokes (1966, pp. 170–71) in his critique of the Downsean model suggested, "position issues" may affect partisan choice far less decisively than "valence issues," which link "parties with some condition that is positively or negatively valued by the electorate." However, the parties' perceived competence to govern and to achieve goals deemed important by the public, or the personal appeal of party leaders, have rarely been introduced as *explanatory* variables into spatial analysis.[1] In multiparty

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[1] Rabinowitz (1978, p. 809), in a spatial analysis of presidential candidates, makes the interesting point that the personal appeal of candidates may be taken into account without adding another dimension to a configuration defined in terms of political issues. He observes that "the more appealing the candidate, the

systems, as Lehmbruch has suggested (1969, p. 299), the definitions of party distances as provided by the elite members of the various parties may greatly shape voter evaluations of parties. Alignments of parties in government, or in opposition, supply perhaps the most visible clue to the voters for judging similarities and distances between parties (Pappi, 1973, p. 202). Even where no party coalition governs, voters may take important cues from party leaders' pronouncements regarding possible alignments with other parties.

This article pursues this suggestion in analyzing the party preference orders of West German voters. The German Federal Republic has been governed by a succession of coalition governments. Since 1961, the pattern of a coalition composed of two parties and opposed —within the Bundestag—by a single party has not been disturbed, although coalition partners have changed. The data to be analyzed consist of preference rankings of the West German parties. The series of surveys of the West German electorate available for analysis makes it possible to extend the proposed inquiry over a considerable span of time (1961–1976).[2] These surveys contain a rich body of comparable preference data on the German parties, providing a good opportunity to address questions of change.

Models of Spatial Analysis: A Brief Comparison

Most attempts at spatial analysis of party systems rely on the "similarities" model (Coombs, 1964, pp. 6, 25–28). What is given in such an analysis is judgments of similarity between stimuli such as parties or candidates, and what emerges from the non-metric analysis of those similarities is distances between those stimuli (Shepard, 1962; Kruskal, 1964). These distances can then be displayed by way of an n-dimensional plot, depending on the number of axes the analyst decides to select. The judgments of similarity providing the input to the analysis, it should be noted, are averages formed across respondents doing the judging. The individual element is averaged out of the picture, literally speaking. Such a step, to be sure, filters out a great deal of "noise" like personal idiosyncrasy or even frivolity, which can be safely considered to be random in nature. But that step also suppresses meaningful differences in the judgments of stimuli, obscures asymmetrical relations between them and produces a solution that need not fit the judgments of a large number of individuals. That individuals rely on a particular dimension for their judgments cannot be established with these kinds of solutions.

The typical spatial analysis, moreover, uses as its point of departure some form of affective rating of stimuli; e.g., how much do you like or dislike a particular party? The judgment of similarity is then inferred by comparing the rating of one party with that of another one, the overall similarity between any two parties being expressed by correlation coefficients or averaged absolute differences between the two ratings. What is submitted for analysis, therefore, consists not of similarities in perception, but of correlations of affect. The resulting configuration reveals the structuring of affect: the larger the distance between any two stimuli, the lower the affect correlation.

Affective ratings, it would seem, are far more akin to preference orders, into which they can easily be translated, than to judgments of similarity. It is one thing to perceive two stimuli as similar or dissimilar and another to prefer or rate one higher than the other. One of the most elegant and intuitively appealing models for the analysis of this type of "data" is furnished by Coombs' "unfolding" model (1964, pp. 80–121). The essence of this model is best expressed in Coombs' own words (1964, p. 80):

> Each individual and each stimulus [i.e., party] may be represented by a point on a common

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more he is located toward the population center; and the less appealing, the farther he is located from the population center." For an attempt at projecting such dimensions as personal appeal or competence into a spatial configuration, see the author's scaling analysis of West German party leaders (Norpoth, 1977; an English version can be obtained from the author).

[2] The data used in this analysis were made available by the *Zentralarchiv für empirische Sozialforschung* in Cologne, Germany, through a joint project with the *Zentrum für Umfragen, Methoden und Analysen* in Mannheim and the ICPSR in Ann Arbor, Michigan. The 1961 survey (ZA no. 056, ICPSR no. 7100) was conducted by Gerhart Baumert, Erwin K. Scheuch and Rudolf Wildenmann; the 1965 survey (ZA no. 556, ICPSR no. 7103) by Max Kaase and Rudolf Wildenmann; the 1969 survey (ZA no. 426/427, ICPSR no. 7098) by Hans D. Klingemann and Franz U. Pappi; the 1972 survey (ZA no. 635/636/637, ICPSR no. 7102) by Manfred Berger, Wolfgang Gibowski, Max Kaase, Dieter Roth, Uwe Schleth and Rudolf Wildenmann; and the 1976 survey (ZA no. 823/824/825, ICPSR no. 7513) by the *Forschungsgruppe Wahlen*, Mannheim. Neither the principal investigators nor the institutions involved in the data preparation bear any responsibility for the analyses or interpretations presented in this article.

> dimension called a *J Scale*, and each individual's preference ordering of the stimuli from most to least preferred corresponds to the rank order of *the absolute distances of the stimulus points* from the ideal point, the nearest being the most preferred. The individual's preference ordering is called an *I Scale* and may be thought of as the *J Scale* folded at the ideal point. . . (italics in original).

This formulation, of course, applies only to the one-dimensional case, but the notion expressed here can also be extended to the multidimensional case.

The unfolding model, as developed by Coombs, is deterministic. There is no provision for error or goodness of fit. A particular preference order (*I scale*) either fits a given ordering of the stimuli (*J scale*) or it does not. For the success of application it does not matter how many individuals select the various *I scales*. At the same time, the fact that just two individuals choose the stimuli in exactly the reverse order is sufficient to postulate the existence of an ordering of stimuli in that fashion. While very much hinges on the choices of very few individuals, little weight is attached to the choices of many. It is difficult, if not impossible, without side information, to screen out the significant from the idiosyncratic choices. The recovery of the metric configuration of both individuals and stimuli in the same space is an arduous task.

These drawbacks notwithstanding, this article takes advantage of the unfolding model for casting light on the *ordering* of stimuli which underlies individual preferential choice. The model is well suited for determining the fit of a given ordering of parties such as the left-right framework. It permits us to separate preference orders which are compatible with a given party ordering from those which are not.

Party Preference Orders: The Three Major Parties

The first part of the analysis is devoted to preferences involving the major parties, that is, the parties represented in the West German federal parliament. Since the same three parties, and no others, have held the seats of the Bundestag from 1961 onwards, it would seem justifiable to examine preferences for them in their own right. To provide a sketch of their identities, let us take note of the social cleavages which spawned these parties (Pappi, 1973, 1976; Kaack, 1971). The oldest among them, the Social Democratic party (SPD), is closely identified with the industrial working class and espouses, in terms of practical policy, a labor union ideology. The chief partisan opponent of the SPD, the Christian Democratic/Social Union (CDU/CSU), is the heir to a party generated by the conflict over the role of the Catholic Church in society. The CDU/CSU continues to articulate policy positions agreeable with the values, if not always the stated positions, of the Catholic Church. It draws its most loyal electoral support from the Catholic part of the public. Perhaps surprisingly, neither the SPD nor the CDU/CSU, as Pappi has pointed out (1976, p. 6), confronts the other one as its chief policy adversary in the domain in which it has staked out its distinctive claim. Instead, it is the Free Democratic party (F.D.P.), heir to the various streams of German liberalism, which performs that role. The F.D.P. and its predecessors have traditionally advocated limited government interference in economic and social matters as well as a reduction of church influence. Its electoral clientele is concentrated among the non-Catholic middle class.

The sharp edges of these policy confrontations have undoubtedly been blunted ever since the current parties or their predecessors began mobilizing electoral support. To many voters, these distinctions have become all but invisible. Nonetheless, the rapport struck between parties and certain groups of voters at an earlier time constitutes a bond of continuing strength and vitality (Pappi, 1976). First preferences for parties, and thus voting choices, are firmly anchored in social affiliations which bear witness to economic and religious cleavages. But what about the *order* of party preferences? How profoundly are second and third preferences shaped by the party locations along the traditional cleavages?

Table 1 presents respondents' preference orders involving the CDU/CSU, SPD, and F.D.P. for the 1961–1976 period. Since the preference rankings were not obtained the same way each year, a brief review of the measures may be in order. In 1961 and 1965 respondents were asked to rate each of the three parties separately on an 11-point scale ranging from −5 (least favorable) to +5 (most favorable). The preference orders were then constructed by comparing the three ratings; ties, consequently, were unavoidable. The 1969 survey affords the analyst the rare opportunity to derive party preference rankings from paired comparisons. Respondents were presented with each pair of parties and asked which of the two they preferred. A respondent had to make ten such comparisons since there were five parties, and from this set of comparisons a respondent's preference order was obtained. Surprisingly few

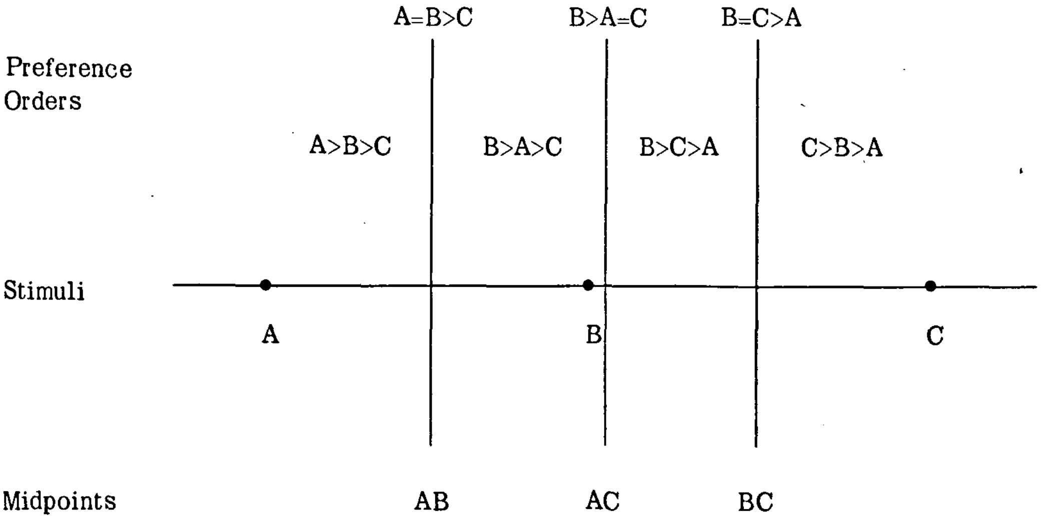
Table 1. Party Preference Orders of the German Electorate, 1961–1976, Pre-Election Surveys (Percent)

Preference Order	July 1961	September 1965	September 1969	September 1972	June 1976
S>C>F	16	20	32	18½	8
S>F>C	8	8	14	33	37
S>C=F	7	8	–	–	–
S=C>F	8	7	–	–	–
C>S>F	19	17	33	28	19
C>F>S	21	18	15	14	28
C>S=F	12	13	–	–	–
C=F>S	4	4	–	–	–
F>S>C	1	1	3	4	6
F>C>S	3	3	3	2½	2
F>C=S	1	1	–	–	–
F=S>C	1	1	–	–	–
Total	101	101	100	100	100
N=	1463	1275	818	1595	1872

Source: Zentralarchiv, Cologne, West Germany, study nos. 056, 556, 426/427, 653/636/637, and 823/824/825; also ICPSR, Ann Arbor, Michigan, study nos. 7100, 7103, 7098, 7102, 7513.

individuals were caught in intransitive rankings—not more than three percent of those making all ten paired comparisons. In 1972 and 1976 respondents were asked to indicate their preference orders for the five parties in a single step, with ties between two or more parties not permitted. At this stage of the analysis, however, only preferences for the major three parties are considered.

Three parties—let us denote them as *A*, *B*, and *C*—can be placed in three different ways: *A–B–C*, *B–A–C*, and *A–C–B* as well as their mirror images; but *C–B–A*, for example, would be considered identical with *A–B–C*. Each of those orderings forms a unidimensional configuration of parties that might underlie the preferential choice of individuals. This notion is illustrated in Figure 1, using the ordering *A–B–C*. The three parties are located on a single axis at the points indicated. Midway between any two points right through the "midpoints" run vertical lines which delineate zones of preference orders. Provided that individuals rank their party preferences by folding



Source: Compiled by the author.

Figure 1. Preference Orders Compatible with One-Dimensional Configuration of Three Stimuli

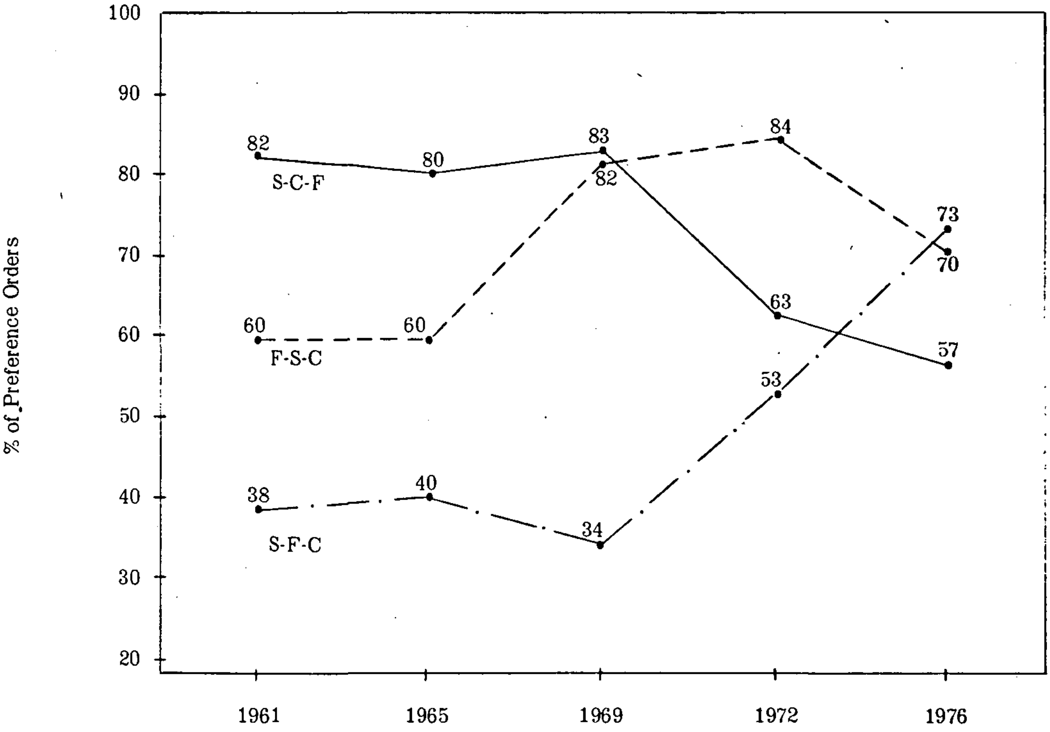
the party continuum at the point of their own location ("ideal point"), four distinct preference orders are possible, given the party ordering shown in Figure 1: *A>B>C* for individuals located to the left of midpoint *AB*, *B>A>C* for those located between the midpoints *AB* and *AC*, *B>C>A* for those located between the midpoints *AC* and *BC*, and *C>B>A* for those located to the right of midpoint *BC*. As can be quickly verified, the preference orders *C>A>B* and *A>C>B* are not permitted by the party ordering in Figure 1. The location of *B* in the middle blocks these two orders. Once ties in rankings are allowed, three further preference orders qualify for the above party ordering: *A=B>C* for individuals located exactly at the midpoint *AB*, *B>A=C* for those at midpoint *AC*, and *B=A>C* for those at midpoint *BC*.

Dimensions of Party Preference Orders

The share of individual preference orders compatible with a given ordering of the three main German parties for the 1961–1976 period is charted in Figure 2. It should be stressed that none of the preference orders listed in Table 1 belongs exclusively to one ordering of the parties. In fact, each preference order is compatible with two of the three orderings. This fact, unfortunately, narrows sharply the leeway for choosing between the competing orderings on the basis of the preference frequencies.

Above all, it is worth noting that no single ordering of parties dominates throughout the whole period. The early 1960s witnessed the dominance of the S–C–F ordering, whereas in the late 1960s the F–S–C ordering rose to prominence. On two occasions, that is in 1969 and 1976, two orderings tied for the lead. What is the message conveyed by this change, and what light does it cast on the meaning of the various party orderings?

Viewed in the light of the above discussion of the German party system, the S–C–F ordering may be construed as the legacy of the economic cleavage, pitting the "socialist" SPD against the "free enterprise" F.D.P. Given the



Source: Zentralarchiv, Cologne, West Germany, study nos. 056, 556, 426/427, 653/636/637, and 823/824/825; also ICPSR, Ann Arbor, Michigan, study nos. 7100, 7103, 7098, 7102, 7513.

Figure 2. Percent of Individual Party Preference Orders Compatible with Different Party Orderings, 1961–1976

prominence of status polarization—some would say, class struggle—in any industrial society, one should not be surprised to find its trace in political orientations and preferences of the mass public. But if so, Figure 2 suggests a marked decline of this dimension in the years since 1969. To be sure, the SPD's decision to abandon socialist ideology and rhetoric helped close the gap between this party and the F.D.P. Yet, this step was taken in the late 1950s whereas, according to Figure 2, the S–C–F curve declined most sharply between 1969 and 1972, that is, a full decade after the SPD had come forward with its major policy reorientation. The magnitude as well as the timing of this decline cast some doubts on the claim that the S–C–F ordering bears out the economic cleavage to the extent suggested by its share of preference orders.

The F–S–C ordering recalls the religious cleavage pitting the "secular" F.D.P. against the "clerical" CDU/CSU, to exaggerate a bit. As Figure 2 indicates, the F–S–C curve rises sharply in 1969 from a moderate level and clearly dominates the other two in 1972. This would suggest intense and exacerbated religious tensions during the late 1960s and early 1970s. Several new issues indeed arose at that time which were fought in the shadow of the religious cleavage, such as the easing of divorce and abortion laws as well as the phasing out of denominational schools. But the overwhelming share claimed by this dimension nonetheless strains credulity. Religious issues have rarely if ever topped the priority list of the public in recent years.

The third, and final, possible ordering of the three German parties produces a pattern that is quickly recognized as the left-right continuum. Klingemann (1972) and Inglehart and Klingemann (1976) have demonstrated that German voters in large numbers place SPD, F.D.P. and CDU/CSU in that order on a left-right continuum. There can be no doubt that the public perceives significant distances between these three parties on such a continuum, at least since 1969; for lack of data, no such claim can be made with the same certainty for the time before that date.

Turning from perception to preference and, again, to Figure 2, one can quickly note that the left-right ordering in all but one of the years studied accounts for the lowest share of individual preference orders. It is also quite noticeable that its share varies the most over those years. While it is not implausible to see the left-right ordering reach its peak in 1976—what with the CDU/CSU's campaign theme of "freedom versus socialism"—it is certainly puzzling to see this ordering decline during the late 1960s. If indeed an ideological orientation possesses the average voter and governs his or her reaction to politics, it manifests itself in a highly erratic fashion. That alone can be safely taken as evidence that left-right ideology does not greatly shape the average voter's party preferences in Germany. Negative findings of this sort were also reported by Butler and Stokes (1969, pp. 200–05) for British voters, whose party system closely resembles the German system.

At this point, the reader may be puzzled by the lack of congruence between the left-right ordering of parties (S–F–C) and their supposed ordering on the economic dimension (S–C–F), which is typically construed in left-right terms. One way of resolving this puzzle, if it is one, would be to argue that these two dimensions are the same, but that the F.D.P. changed its location in the party system, moving from the right of the CDU/CSU to the middle position between SPD and CDU/CSU. Between 1966 and 1969, the F.D.P. indeed underwent a sweeping transformation including a leadership shake-up and the adoption of a platform advocating wide-ranging political reform. Both of these moves were widely regarded as a shift of the F.D.P. (until then simply the FDP) to the left of the CDU/CSU, if not the SPD (Kaltefleiter, 1970; Zülch, 1973). Granted that such a shift occurred, the public nonetheless had yet to take note of it by 1969. Judging from respondents' preferential choice, the S–F–C ordering, which features the new location of the F.D.P. in the middle, fails to increase its share in 1969. That share, in fact, is lower than ever, whereas the alternative S–C–F claims a share in that year that is barely diminished compared to 1965. Voters' preferential choices, then, registered little, if any, movement of the F.D.P. to the left (of the CDU/CSU) before the 1969 election.

Coalition Alignments and Party Preference Orders

The efforts to decode the meaning of the three competing party orderings in terms of social cleavages or left-right ideology have not proven very successful. It is highly unlikely that these dimensions shifted so frequently and so strongly during the 16 years from 1961 and 1976 as to account for the changing shares of the three party orderings. It is even less likely that throughout that period one and the same dimension was underlying most preference orders, but that the ordering of parties on that dimension changed. What, then, accounts for the changing shares of preference orders? While party positions and the salience of the various cleavages may have changed somewhat over time, far more visible change has occurred in the coalition alignments among the parties—from a CDU/CSU–F.D.P. coalition until 1966, through a CDU/CSU–SPD coalition between 1966 and 1969, to a SPD–F.D.P. coalition since 1969. What impact did these realignments exert on the voters' party preference orders?

The best opportunity for such a test is provided by the 1969 panel survey. In 1969, immediately following the federal election, the SPD entered into a coalition with the F.D.P., thereby dissolving the coalition it had maintained until then with the CDU/CSU. While the pre-election interviews took place at a time when the CDU/CSU–SPD coalition was still in office,[3] the post-election interviews were held right after the new coalition had been initialled. Table 2–a documents the massive reshuffling of second and third preferences among SPD partisans during the short six weeks which elapsed between the two survey waves. Whereas before the election 65 percent of the Social Democrats favored the CDU/CSU secondmost, only 45 percent did so afterwards with the majority now favoring the F.D.P. Almost half of the 65 percent forsook the CDU/CSU for the F.D.P., and exactly half of the post-election S>F>C champions adopted their preference order in the wake of the party leadership switch of coalition partners.

The net shift attributable to this switch in the aftermath of the 1969 election amounts to 20 percent. By 1972 it had increased to 30 percent and by 1976 had reached the 50 percent mark. While the later shifts cannot be fully attributed to the coalition's existence and persistence, the near-instantaneous shift occurring in 1969 is surely an impressive tribute to the impact of elite coalition decisions on the preference orders of the electorate. A shift of similar magnitude can also be found among CDU/CSU partisans during the time between

Table 2. Change in Party Preference Orders among SPD Partisans in 1969 and CDU/CSU Partisans in 1965–1969

a. Social Democrats, 1969

Pre-Election 1969	Post-Election 1969 S>C>F	S>C>F	Total	
S>C>F	37	28	65	
S>F>C	8	27	35	
Total	45	55	100%	(n=223)

b. Christian Democrats, 1965–1969

1965	1969 C>F>S	C>S>F	Total
C>F>S	–[a]	–	52
C>S>F	–	–	48
Total	31	69	100%

Source: Zentralarchiv, Cologne, West Germany, study nos. 556 and 426/427; also ICPSR, Ann Arbor, Michigan, study nos. 7103 and 7098.

[a] Internal frequencies not available for lack of panel data.

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[3] It must be noted, however, that the coalition signals issued by party leaders throughout 1969 were somewhat confusing. It was an open secret that CDU/CSU as well as SPD, even though they were governing together, were each prepared to enter into a small coalition with the F.D.P. after the election. The "coalition" between SPD and F.D.P. during the election of Heinemann (SPD) as federal president in early 1969 offered an unmistakable clue to the prospect of a government coalition between these two parties. Yet, despite such clues, the supporters of CDU/CSU and SPD in the electorate maintained a preference for this coalition and close affection for each other's party.

1965 and 1969 when their party switched coalition partners (see Table 2–b).

If coalition realignments change party preference orders, these alignments themselves must be considered as among the forces, along with the social cleavages, which *shape* party preference orders. The finding from the 1969 survey might warrant the estimate that *at least* half of those individuals whose preferences orders are compatible with a given coalition alignment are formed in response to that alignment. Given the short time interval in 1969 and the evidence of more change subsequently, this estimate may be viewed as conservative. Using the 50 percent mark, I have recalculated the shares of the three party orderings in 1969, subtracting from each of them (see Figure 2) half of the frequency of those preference orders which were compatible with the prevailing coalition alignment.

This adjustment for the coalition effect in 1969 cuts the shares of the two top orderings S–C–F and F–S–C from about 80 down to 50 percent, while leaving the one-third share of the left-right ordering S–F–C undisturbed. Even now, because of the large overlap between their shares, the orderings S–C–F and F–S–C make inflated claims. While it is impossible with the available data to estimate the exact shares of the two dimensions suggested by these two orderings, namely class and religion, each of them has a close, and perhaps superior, rival in the cues emanating from the coalition decisions of party elites.

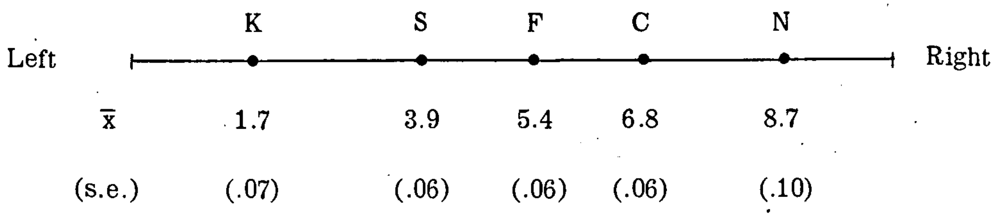
Coalition realignments go a long way toward accounting for the fluctuations of the preference shares observed in Figure 2. The sharp decline, for example, of the S–C–F ordering after 1969 coincides with the establishment of a party coalition (SPD and F.D.P.) which violates that ordering. Likewise, the ordering which surges during the same time, that is, S–F–C, features the new partners side by side; so does the alternative F–S–C, whose share remains stable. As a rule, the dominant party ordering at any given time invariably witnesses the two coalition partners in adjacent positions.

It cannot be overlooked, however, that on several occasions the least popular party ordering also features the coalition partners side by side. That holds true for the ordering S–F–C in 1961 and 1965 when it was compatible with the CDU/CSU–FDP coalition and in 1972 with respect to the SPD–F.D.P. coalition. The low share of this ordering may be viewed as further testimony to the small significance, until recently, of the left-right continuum, with which it is consistent. At the same time, coalition politics offers an important clue which may help resolve the paradox noted above. That clue refers to the coalition strategy of the opposition party. Although opposition parties are often ambiguous about their choice of a most-preferred coalition partner, it is safe to say that the SPD during the early 1960s aimed at a coalition with the CDU/CSU, which ultimately was concluded in 1966. While the leading ordering (S–C–F) happens to accommodate both the coalition between CDU/CSU and F.D.P. and the affinity of the SPD to the CDU/CSU, the S–F–C ordering does not. Likewise the surge to prominence of the S–F–C ordering between 1972 and 1976 coincides with a move, however cautious and grudging, on the part of CDU leaders toward the F.D.P. as their preferred coalition partner in the future. That move would well account for the massive shift among CDU/CSU voters from the SPD to the F.D.P., as far as second preferences are concerned. On the whole, the dominant party ordering which underlies preferential choice in West Germany turns out to be the one which features the coalition partners side by side and the opposition party alongside that party which is targeted by the leaders of the opposition party as future coalition partner.

Party Preference Orders:
The Five-Party Case (1969–1972)

The three parties treated in this analysis so far have dominated electoral and legislative politics in the Federal Republic. But below the five-percent level of electoral support, a few minor parties continue to exist and occasionally threaten the tranquil alignments in the Bundestag. The near-miss of the National Democratic Party (NPD) coupled with the bare success of the F.D.P. in 1969 brought the parties close to the brink of a major governmental crisis. While less potent at the polls, the German Communist party (DKP) remains visible largely as the result of its vocal support on university campuses and because of the issue of public service employment of left-wing radicals. These two parties have added color, contrast and unrest, however unwelcome, to the mainstream politics offered by the three major parties.

By far the most familiar continuum on which each of the five parties can be assigned a singular location is the left-right framework. Voters in Germany, as is also the case in Italy and France (Barnes and Pierce, 1971), are capable of viewing their parties in left-right terms. They also agree to a great extent on the placement of the parties from left to right. Figure 3 reproduces Klingemann's (1972) findings concerning the five parties' mean locations



Source: Hans Klingemann (1972), "Testing the Left-Right Continuum on a Sample of German Voters," *Comparative Political Studies* 5:96.

Figure 3. Left-Right Locations of German Parties as Perceived by Mass Public, 1970

in the eye of the mass public.

Provided that a voter arrives at a preference order by folding the left-right continuum at the point of his or her own location on it and by picking the parties in the order from least to most distant, 11 distinct preference orders involving 5 parties are possible (Coombs, 1964, p. 87). That assumes that voters are unanimous in their positioning of each party. If we relax this unrealistic assumption and allow for some disagreement so long as the left-to-right *ordering* of the parties is not disturbed, 5 more preference orders are permissible for a total of 16.[4] How large a share of all individual preference orders, weighted by the number of respondents picking them, do these 16 orders claim?

Table 3 lists the respondents' preference orders involving all five parties for 1969 and 1972, for which such data were available. As can be verified with the information in Table 3, 34 percent of respondents in 1969 and 53 percent in 1972 preferred the parties in one of those 16 orders permitted by the left-right ordering. The two figures are virtually identical with those found for the left-right framework in the three-party analysis. It appears, then, that the addition of two parties located at one or the other end of the left-right continuum has neither diminished nor enhanced the share of that framework. But let us take a closer look at the preference orders in the five-party case

It is quite apparent from a quick scan of the listing in Table 3 that preference orders are distributed in a highly skewed fashion. The top ten orders, in terms of frequency of choice, account for 88 percent of individual choices in 1969 and 1972. These ten preference orders invariably feature the three main parties in the first three ranks with the NPD and the DKP (ADF in 1969) alternating between the last two ranks.[5] One could suppose, therefore, that the fit of the left-right continuum, limited as it is already, is rather fortuitous.

An ordering of parties featuring the three main parties in the left-right fashion but reversing the locations of NPD and DKP happens to claim 32 percent of individual preferences in 1969 and 49 percent in 1972. In other words, the rather absurd ordering, N–S–F–C–K, as Table 4 bears out, does just about as well as the much-heralded left-right ordering, K–S–F–C–N. And that holds for both 1969 and 1972. These findings severely challenge the significance of the left-right framework as a guide to preferential choice in the German mass public.

Most respondents appear to treat NPD and DKP as if they were interchangeable, albeit undesirable, parties. The 1969 panel survey consisting of a pre- and post-election wave reveals that one of every three respondents moved between NPD and DKP, as far as ranks four and five, that is last and next to last, were concerned, not to mention the 20 percent of respondents who would not assign separate ranks to them. The fact that about as many respondents shift from NPD to DKP as do the other way around suggests that change was largely of a random nature, like the flip of coin. As a result, as many as two-thirds of the respondents might be inclined to treat these

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[4] The 16 preference orders, with the most-preferred party listed first each time, are: KSFCN, SKFCN, SFKCN, SFCKN, SFCNK, FSKCN, FSCKN, FSCNK, FCSKN, FCSNK, FCNSK, CFSKN, CFSNK, CFNSK, CNFSK, NCFSK. Note that in this section of the article tied ranks are not considered. Respondents in the 1969 and 1972 surveys were not given the option to tei two or more parties for a particular rank.

[5] From now on, the symbol "DKP" or simply "K" will be used to refer to the Action Democratic Progress (ADF) in 1969 as well as to the DKP in 1972. The ADF was an electoral alliance between the DKP and the German Peace Union (DFU).

Table 3. Party Preference Orders of West German Electorate, 1969 and 1972

Party Preference Order	Frequency		Party Preference Order (continued)	Frequency	
	1969	1972		1969	1972
S F C N K	29	315	C S K N F	–	2
S F C K N	85	210	C N K S F	–	1
S F N C K	1	13	C F N K S	1	–
S F K C N	3	45	C F K S N	2	–
S F N K C	–	25	F S C N K	6	35
S F K N C	1	9	F S C K N	16	33
S C F N K	122	201	F S K C N	1	3
S C F K N	141	94	F S K N C	–	2
S K F C N	3	13	F C S N K	19	31
S N F K C	–	1	F C S K N	5	10
S K F N C	–	1	F N S C K	–	2
S C N F K	6	7	F C N S K	–	1
S N C F K	2	1	F C K S N	2	–
S C K N F	–	1	F K N S C	1	–
S N C K F	–	1	F S N C K	1	–
S N K C F	–	1	N K C F S	–	1
S C K F N	6	–	N F S C K	–	1
S K N F C	1	–	N S F C K	–	2
S K C F N	3	–	N C F S K	6	1
S K C N F	1	–	N S C F K	–	1
C F S K N	56	40	N C S F K	1	1
C F S N K	66	178	N S F K C	1	–
C F N S K	10	17	N F S K C	1	–
C F K N S	2	2	N K S C F	1	–
C S F K N	135	97	N K F S C	4	–
C S F N K	138	351	K N F C S	–	1
C N F S K	3	9	K S F N C	–	1
C N F K S	–	2	K S F C N	2	4
C S K F N	2	2	K C S F N	1	–
C S N F K	11	6	K S C F N	1	–
C K S F N	1	1	K F S C N	1	–
C N S F K	5	7	K N F S C	1	–
C N K F S	–	2	Total	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.

Key: C = Christian Democratic Union/Christian Social Union
S = Social Democratic party
F = Free Democratic party
N = National Democratic party
K = Communist party (1972), Action Democratic Progress (1969)

two parties as interchangeable.

Neither in 1969 nor in 1972 does the left-right ordering account for most individual preferences involving all five parties; nor does it even come close to such a standing. Table 5 lists the four party orderings which account for the highest number of preferences. To be sure, two of them closely approximate the left-right ordering. The K–S–C–F–N ordering simply has C and F reversed, that is, the CDU/CSU and the F.D.P. Likewise, the K–F–S–C–N ordering has S and F reversed. These two party scales might be construed as slightly impure versions of the left-right continuum. One could indeed make a case for the F.D.P. being located to the right of the CDU/CSU up until about 1969 when the party moved leftwards. It is conceivable that left-right considerations govern voters' preferential choices except for the fact that not all voters link parties with those terms in exactly the same fashion.

Even without probing people's understanding of the left-right concept, we can soundly reject that hypothesis. It is just too apparent from Table 5 that the slightly impure left-right orderings (K–S–C–F–N and K–F–S–C–N) happen to account for no larger a share of preferences than do two alternative orderings

Table 4. Left-Right Party Ordering and Alternative: Share of Individual Preference Orders

Party Ordering	Percent of Individuals Whose Preference Orders Are Compatible with Party Ordering 1969	1972
K–S–F–C–N	34	53
N–S–F–C–K[a]	32	49
N =	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.

[a] On this party ordering, the positions of K and N as featured on the left-right ordering are reversed.

where the locations of NPD and DKP are reversed. It simply does not make a difference whether it is the NPD or the DKP which is placed at the right (or the left) end of the party continuum. These two parties seem to surround the three-party field like, two planets circling around the sun. They lack fixed locations in a configuration featuring all five parties. The left-right ordering is no more than a transient constellation of the five parties which may be occasionally observed.

These results suggest that preferential choice involving all five parties is mostly guided by the consideration of the *political* status of the parties, with the established and legislatively represented parties on one hand and the parties outside the Bundestag and despised by the parties inside, on the other. What matters most to the average voter in making a preferential choice, it seems, is not so much the policy position(s) or ideology of the minor parties but the fact that they are rejected without qualification as possible allies by each of the parties which the voter might most prefer. SPD leaders find the ideologically more related DKP no more acceptable as an ally than the ideologically more distant NPD. And the same holds true for the CDU/CSU leaders as well as the F.D.P. leaders. It is clues of this sort, rather than perceptions of policy locations, which shape preferential choice among the German public.[6]

Conclusions

This article has been mainly concerned with the question of preferential choice. Which considerations govern the order in which German voters prefer their parties? In particular,

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[6] An alternative view based on "rational choice" premises has been suggested to me by G. Robert Boynton. This view maintains that since both NPD and DKP are so unlikely to receive enough votes, that is, more than five percent, to win seats in the Bundestag, votes for these parties would be "wasted" and, therefore, are not cast. While this may account for the low support for these parties at the polls, it is not clear why this should also prevent voters from expressing their true preference for these parties. In any event, the NPD during the years 1966–69 mustered sufficient strength in state elections to enter state legislatures and came close enough in the 1969 federal election, with 4.3 percent of the vote, to make a vote for it worthwhile.

Table 5. Other Party Orderings Featuring the Minor Parties at Endpoints: Share of Individual Preference Orders

Party Ordering	Percent of Individuals Whose Preference Orders Are Compatible with Party Ordering 1969	1972
K–S–C–F–N	78	57
N–S–C–F–K	78	57
–	–	–
K–F–S–C–N	77	75
N–F–S–C–K	75	76
N =	907	1785

Source: Zentralarchiv, Cologne, West Germany, study nos. 426/427 and 635/636/637; also ICPSR, Ann Arbor, Michigan, study nos. 7098 and 7102.

does an issue-defined configuration of parties underlie preferential choice? Most spatial analyses of voter evaluations (and preferences) of parties assume a common political space whose dimensions are typically construed in terms of policy issues or ideology.

The analysis above has tried to isolate the dimensions relevant to party conflict in Germany and to estimate their shares of preference orders. The results of the analysis dealing with mass preferences for the three parties represented in the Bundestag reveal traces of the two classical cleavages, class and religion, which constitute the main long-term determinants of electoral choice. What is noteworthy, however, is the fact that throughout the 16 years covered (1961–1976) the apparent contributions of these dimensions vary greatly. These fluctuations, in fact, closely parallel changes in the coalition alignments of the parties. The ordering of party preferences is highly susceptible to the coalition cues provided by party elites.

This conclusion is not challenged by an analysis of preferences involving the two minor parties NPD and DKP along with the three major parties. Any major party ordering flanked by the minor parties does about equally well accounting for preferential choice, irrespective of the side on which the minor parties are placed. Since this holds true for each of the three party orderings involving the major parties, including the left-right ordering, it is highly unlikely that a spatial configuration defined in terms of position issues or ideology underlies the preferential choice of German voters. Rather, it appears that a sharp dichotomy separates the major parties from the minor ones and that this dichotomy is defined by the political status of the parties.

The preferential choice of voters, to a large extent, is based on cues taken from party leaders. Through actions or doctrine, these leaders define the relationship between their own party and the other parties. Some of the other parties are summarily ruled out of contention as possible allies by being declared "unconstitutional" or "extreme." And from among the others, one or more are selected as the most preferred ally or allies. The policy dimensions which may shape those elite decisions are not at issue here. They do not greatly affect the preferential choice of average German voters. What matters more to voters are those decisions themselves and the cues they provide.

How relevant are these conclusions to preferential choice in other multiparty polities? The German case may seem of limited value since only three parties are represented in the elected assembly. However, several studies dealing with more extended multiparty systems suggest mass susceptibility to elite cues regarding preferential choice there as well. In their study of party distances in Norway, for example, Converse and Valen report (1971, p. 146) that between 1965 and 1969 three parties converged on a single point on the perceptual map of the public as reconstructed by multidimensional scaling. This shift is attributed to the formation of a government coalition involving those three parties. Likewise, Damgaard and Rusk (1976), in their analysis of perceived party distances in Denmark, point to informational cues such as the parties' coalition behavior, rather than to "sophisticated belief systems," in accounting for the close fit between mass evaluations of parties and the left-right framework (Damgaard and Rusk, 1976, p. 196).

The fact that in West Germany and elsewhere voters' preferential choices are considerably affected by party elite cues raises the question of popular representation in multiparty systems. In most of those systems, the government consists of a coalition of several parties. Popular influence may be facilitated by parties forming that kind of coalition most preferred by the electorate. But what if popular coalition preferences—like party preference orders—are guided by the preferences and actions of the very party elites forming the coalitions? How much influence can the electorate exercise under these circumstances? These are challenging questions worthy of closer scrutiny.

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Political Periods and Political Participation*

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Analysis of complementary data sets, a 1965–1973 panel study of young adults and their parents and the 1956–1976 Michigan presidential election series, shows that the late 1960s and early 1970s were a deviant period where participation in American politics was concerned. During this time, the young were more active politically than their elders, substantially increasing their participation from previous years, and Americans on the ideological left participated more than those at other positions along the ideological continuum. While this surge of left-wing activism was not restricted to the young, it probably accounts for the relative participation advantage enjoyed by the young. These findings challenge the "conventional wisdom" about patterns of participation in America. They are best explained by recognizing that the opportunities for political action among the American citizenry are not fixed, but instead vary with changes in the political stimuli across different periods.

The late 1960s and early 1970s were years of exceptional turmoil in American politics. Prominent amid this turmoil—in the anti-Vietnam War protests, the civil rights movement, and the political campaigns of McCarthy, McGovern, and others—were the young, accused by many of being the instigators of the turmoil, if not its underlying cause. The allegedly exceptional activity of the young suggests that the "normal" relationship between age and rate of participation may not have prevailed during these years. The apparent leftward bias of youthful participation seems an exception to the "normal" conservative bias in participation. Careful examination of the participation of this young generation is, therefore, more than a heuristic exercise. It promises to illuminate and possibly explain a potentially deviant case of participation. It can as well provide a hard test of current theories of participation.

For this inquiry we draw initially upon data from the second wave of a two-wave panel study of young Americans and their parents.[1] The first wave was a representative cross-section sample of 1,669 high school seniors and their parents, personally interviewed in the spring of 1965. The second wave, conducted in early 1973, consisted of personal interviews with 1,119 of the youths (now 25 and 26 years of age) and 1,118 of their parents as well as responses to mail questionnaires from 229 youths and 61 parents.[2]

These data are particularly well suited for examining the participation of young adults during the late 1960s and early 1970s. To begin with, the data provide information on the political participation of emergent adults, precisely the group located in the center of the turmoil in the years prior to 1973. Another advantage of these materials is that they embrace a wide range of political activities, both electoral and non-electoral. They enable us to chart the participation of young adults from before their entry into the adult electorate to their mid-twenties. Finally, the parent data

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[1] The study was conducted by the Center for Political Studies at the University of Michigan. For earlier reports using the same data base, see Jennings and Niemi (1975, 1978).

[2] The total number of personal and mail re-interviews yields an unusually high re-interview rate for each generation, given the eight-year interval between waves: 81 percent for the younger generation and 75 percent for the parents. Detailed comparisons, based on 1965 characteristics, between those re-interviewed and those not re-interviewed indicated very little bias in the 1973 subset.

provide a convenient benchmark against which to gauge the political activity of the young adults. The participation of the parents was charted for the same period and may be compared directly with that of the young. While parents are not the only useful benchmark for comparison and are at a different stage of the life cycle, comparisons across these biologically linked generations have the advantages of holding constant many important factors contributing to participation and of dealing directly with differences between two generations widely regarded as lying at the core of the generation gap during this time.[3]

The two-generation data are the most comprehensive data on participation for the period of interest because they tap a wide range of political activities. Yet they cannot give us a picture of participation at other age levels in these years or for all ages in preceding and subsequent years—two other useful benchmarks for comparison. To provide this perspective, we shall turn subsequently to information on participation from the presidential election series of the University of Michigan's Survey Research Center/Center for Political Studies. These materials enable us to assess the representativeness of the youth and parent samples for their respective age groups, to determine the age-participation relationships across all age groups in the late 1960s and early 1970s, and to extend our analyses to other time periods. They are limited to campaign-related activities, however, thus restricting the range of our comparisons.

Taken together, the two data sources will allow us to say a good deal about political participation, particularly among the young, during a turbulent period in American politics. Comparing the participation of young adults with that of their parents and of other age cohorts at the same time, and with that of the same age cohort in other times, will triangulate upon what was normal and what exceptional about these young. Identifying deviance in this generation, though, is not the same as explaining it. While our data are less amenable to the task of explanation, we shall nonetheless elaborate the proposition that strong period forces affected participation during the late 1960s and early 1970s.[4]

Youth-Parent Differences in Rates of Participation

Our initial comparison between the youths and their parents focuses on their rates of participation—that is, how politically active they were between 1965 and 1973. All previous studies have found the same general relationship between age and participation. Participation is lowest for young adults, peaks in the middle-aged years, and then falls off for the old (Milbrath and Goel, 1977, pp. 114–15). Verba and Nie (1972, p. 141) have shown, further, that lower rates among the old may be attributed to their lower socioeconomic status and that, once status differences are removed, participation rises gradually with age through age 65, and then levels off afterwards. These results give us every reason to expect substantially greater political participation in the parental generation than in the youth generation.

The political activity of the parents and youths in our sample was gauged by answers to nine questions covering a variety of participative acts. Five questions dealt with involvement in election campaigns since 1965, including referenda and contests for public office at any level. They elicited information regarding the following: persuading others to vote a certain way, attending meetings or rallies, displaying buttons or bumper stickers, donating money, and doing other kinds of campaign work. The four remaining questions covered non-electoral activities performed at any previous time: writing a letter to the editor, contacting a public official, engaging in protests or demonstrations, and working with others to solve some community problem.[5]

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[3] Our use of the term *generation* refers primarily to the lineage concept rather than the age or birth cohort concept. Actually, the youth sample is, aside from dropouts, representative of the 1947–1948 birth cohort eligible for the first grade in 1953. The parent sample is, strictly speaking, only representative of parents of high school seniors, although comparisons between the parents and similarly-aged respondents in cross-section samples reveal strong political similarities. For a useful explication of the differences between the lineage and age cohort models see Cutler (1976, 1977).

[4] An extensive discussion of period effects, as well as other sources of discontinuity between age cohorts, may be found in Riley, Johnson, and Foner (1972), especially Ch. 2, and in shorter form in Riley (1973). See Jennings and Niemi (1975) for an examination of other types of period effects in the parent-youth pair data set.

[5] For all specific forms of electoral and non-electoral activities, precise information was elicited concerning the timing and nature of the activity. Although the time span specified for the non-electoral acts was not restricted to the 1965–1973 period, in

Separate factor analyses of the activity intercorrelations for the youth and parent samples justify using the same unidimensional index of participation for each generation, in which participation is measured simply by the number of activities performed. The principal components solutions from these analyses are presented in Table 1. The solutions are strikingly similar. For the youth only one factor emerges. This factor explains 26 percent of the *total* variance in the activities, and all activities are substantially correlated ($r \geqslant .35$) with it. For the parents too the principal components analysis yields a single factor. This factor accounts for 29 percent of the *total* variance in parent activities, and all activities but protest are substantially correlated ($r \geqslant .43$) with it. Even though protest does not load highly on this dimension for the parents, it is associated with the other activities. Thus, the underlying structure of participation is virtually the same for the two generations. Lowering the threshold for inclusion of additional factors and oblique rotations of the resulting reference axes distinguish slightly different patterns of participation for the two generations, but we shall use the results at the more general level of participation where a single overall dimension is uncovered.[6]

When the means of the participation index for youths and parents are compared, the young are found to have been more active by a considerable margin. They performed an average of 2.1 activities, compared with 1.7 for their parents. That the youth enjoy this edge is contrary to the findings from previous studies of participation. Part of their edge results from the markedly higher level of protest activity among the young. Six times as many youths as parents were involved in protests or demonstrations from 1965 to 1973. One in six members of this early adult cohort engaged in such direct action, impressive evidence indeed for the wide

Table 1. Factor Analysis of Participation Variables*

	Youth Generation		Parent Generation	
Variable	Factor One	Percent Explained of Item Variance	Factor One	Percent Explained of Item Variance
INFLUENCE	.51	26	.64	41
RALLIES	.63	40	.68	46
OTHER	.58	34	.54	29
BUTTONS	.52	27	.50	25
DONATIONS	.55	30	.59	35
CONTACTS	.53	28	.59	35
LETTERS	.44	19	.43	18
COMMUNITY	.35	12	.53	28
PROTEST	.44	19	.24	6
Percent Explained of Total Variance		26		29

Source: 1973 Parent-Child Socialization Study, Center for Political Studies, University of Michigan

*These are the principal components solutions in which the number of factors allowed to emerge was restricted to those which explained at least the equivalent of the variance in a single item (1/9 or 11.1 percent of the total variance).

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practice virtually all of the youth participation fell into that time span as did over three-fourths of the parental activities cited. Given the age of the filial respondents, the bulk of their electoral activities clustered in the 1972 races.

[6] The results of our principal components analyses are remarkably similar to those reported by Verba and Nie (1972, p. 68) in their 1967 study of participation, in spite of the differences in activities used, populations sampled, and time frames adopted for performance of the activities. Their first factor explains 27 percent of the total variance and correlates substantially with all activities (including voting activities which were omitted from our analysis) except the two reflecting personalized contact. Verba and Nie used their results to justify construction of a summary index of participation from all the items, just as we have done. Unlike us, though, they have moved beyond this first general factor by rotating the reference axes to establish four basic subtypes of participation–voting, campaigning, communal, and particularized contacting–for use in subsequent analysis along with the overall measure.

dispersion of non-conventional behavior among the young during the period. Not surprisingly, the frequency of these actions varied directly with education—rising from 5 percent among those not attending college, to 15 percent among those with some college, and to 29 percent among those with a college degree.

But the participation advantage of the young has a broader base than confrontation politics. The young also enjoyed a substantial advantage on two other activities—attempting to influence the votes of others (47 to 31 percent) and displaying campaign stickers and buttons (30 to 21 percent). These behaviors are directed towards influencing elections, central institutions of conventional democratic politics. That the younger generation has an edge here demonstrates that it was also quite adept at playing the normal political game.

Generational differences in participation rates were minor across the other specific activities. The young outperformed their parents slightly on attendance at rallies (22 to 19 percent), other campaign activities (13 to 11 percent), contacting officials (31 to 30 percent), and writing letters to the editor (6 to 5 percent). Parents enjoyed an edge on only two, donating money for use in campaigns (21 to 17 percent) and engaging in cooperative community service activities (33 to 28 percent). Parental advantages here should hardly be surprising, even in the face of parental lags elsewhere. The parental generation is at the peak of its earning power and in a much better financial situation to contribute to campaigns. Similarly, the parents are more likely to have entered the mainstream of community life and, as a result, to have had greater opportunities to engage in community-betterment activities. Finally, since performance of the noncampaign activities is measured over the respondent's lifetime, the parents have had far more opportunity for participation than their offspring. Given all of these life-stage facilitators, it is surprising that the parental edge is so meager for these activities.

Before interpreting our findings further, however, we need to consider two plausible reasons for the surprisingly greater participation by the youth. One is that previous studies of participation, because they typically included voting with the other forms of activity, may have overstated (or even reversed) age cohort differences. The overall index of participation used by Verba and Nie (1972, p. 141) in their analysis of age effects, for example, contains four voting items and thus builds in a sizable advantage for the older cohorts who vote at much higher rates than recent entrants to the electorate (Hout and Knoke, 1975). Our analysis of the data from the Verba and Nie study, though, demonstrates that a pronounced older adult advantage persists even when voting activities are removed from the participation index. On each of the nine specific activities which parallel those used in our analysis (influence, rallies, donations, membership in a political group, other campaign work, two community activities, and two kinds of contacts), respondents in the age cohorts encompassing the parents (age 45 to 65) outperformed those in the young adult cohort (under age 26) in the years up to 1967. The youthful edge found in our data for the 1965–1973 period deviates markedly from this pattern.

A second plausible explanation for the relatively greater participation by the young adults in our sample is that our findings are an artifact of the restricted populations we have sampled. The high school seniors of 1965 and their parents were not representative of their respective age cohorts. The high school senior population comprised about three-quarters of all people at that age and the top three-quarters where education was concerned. Likewise, the parents were better educated than all parents whose children were of high school senior age in 1965. But this explanation too is discounted when subjected to examination. We compared the *election campaign* activity of young adults (21–29 years of age) with that of parent-aged adults (44–64 years of age, the age group which included three-quarters of the parents), using the Center for Political Studies national election surveys in 1968 and 1972, the two presidential election years in the 1965–1973 period. In both years the young participated more than the parental-age cohort by a noticeable margin. The average activity edge for the youth over the five indicators used (influence, rallies, donations, buttons, and other activity) was .58 to .49 in 1968 and .68 to .54 in 1972. Even though the election studies data did not include the full range of political activities present in our youth-parent data, the results of this analysis add strong support to our findings.

There should by now be little doubt that the surprising advantage in political participation enjoyed by the young adults in the late 1960s and early 1970s over their parents was real. The generation that came of age in the late 1960s was indeed exceptional in its political activity. The political events and turmoil of the period most likely drew these young adults into political activism at rates much higher than normal for young people.

Youth-Parent Differences in Bias of Participation

Typically, only a minority of citizens perform any one of the political activities considered here. That more people are politically inactive than active underscores the extensive slack in use of political resources in America (Dahl, 1961, pp. 271–301). It also enhances greatly the potential for disproportionate participation by certain groups that has the effect of biasing the nature of political representation (Verba and Nie, 1972; Nie and Verba, 1975). The direction of bias may be the same over time, as the differential usage of resources remains constant across various groups. Such is undoubtedly the case in the United States where socioeconomic status is concerned. Alternatively, the direction of bias may vary over time, as groups change their levels of activity. The long history of white activity advantage in the South, now reduced significantly by upsurges in black participation, illustrates the temporal variability of bias.

The most important biases are those that emerge when groups richer in political resources hold policy preferences different from groups with fewer resources. In their 1967 survey of political participation in the United States, Verba and Nie (1972, Ch. 15) found that political activists were noticeably more conservative than the general public. Turning the causal ordering of the relationship around, they also discovered indirect evidence of a tendency for conservatives to be more active (pp. 224–28). In short, the Verba and Nie study shows conservatism and activism to be positively correlated at the beginning of the period under consideration in our study. By implication, this is the usual pattern.

The events of the late 1960s and early 1970s provide some reason for challenging the constancy of this relationship, at least with respect to young adults. Political activity of the young during this period appeared to be focused heavily on left-wing causes and candidates. So pronounced did the tendency towards left-oriented activism among the young seem to be that careful students of public opinion warned us not to assume that the liberal activists were necessarily representative of their more quiescent peers (Converse and Schuman, 1970). Middle-aged cohort political energies, by contrast, did not appear to be directed disproportionately towards left-wing causes and candidates. Thus, the prevailing portrait of the period was one in which more liberal youths confronted their more conservative parents.

Of course, there is one way in which the apparent liberal tendencies of the young activists during this period could be made to square with findings from previous research. Our data show that the younger generation was considerably more liberal than the parental generation (see the marginals for Figure 1 below). It is conceivable that left-oriented participation among the young was so conspicuous because the young were unusually liberal as an age cohort, even while the relatively more conservative youth among them held the "normal" activity edge over their peers. This would not be the first time that a genuine compositional change was mistaken for a change in the functional relationship between two variables.

We have tested for the ideological bias in the political participation of each generation by comparing the participation levels of those who took different positions on major domestic policy issues of the period. Bias refers to the differences in participation rates across various ideological positions, treating political activity as if it is dependent upon ideology. If those at each position participate to the same extent, there is no participation bias to ideology, and the participants are genuinely representative of the entire sample from an ideological perspective. As the levels of participation diverge by ideological position, on the other hand, bias increases. Measured in this way, the bias of participation can be compared across groups with different ideological compositions. (See the appendix for the difference between this measure and the one used by Verba and Nie.)

Assessment of participation bias requires measurement of two concepts—policy orientation and participation. Our measure of participation is the index of political participation described earlier. To measure ideology, we have constructed an index of liberalism-conservatism based upon the positions of respondents in 1973 on a series of important domestic issues: government guarantees of jobs, protection of rights of the accused, government aid to minorities, legalization of marijuana, school busing, equal rights for women, and changes in the form of government.[7] Positions on each of these issues were initially recorded on a seven-point scale, which we classified as liberal (1–3), moderate (4), and conservative (5–7) for purposes of our analysis. An index was formed across the issues by simply subtracting the number of conservative positions from the number of liberal positions. For this presentation, the resulting index score has been collapsed into five categories: strong liberal (+7 to +3), weak liberal (+2 to +1), moderate (0), weak conservative (–1 to –2), and strong conservative (–3 to –7).[8] We could not include foreign policy issues in this index because too few of them were covered in the survey instrument and none was asked in terms of the seven-point scale. Using actual positions on issues is, of course, only one of several legitimate ways to measure ideology (Converse, 1964). This measure is sensitive to constraint among issue positions, but it is indifferent to whether the sources of that constraint lie in some overarching belief system.

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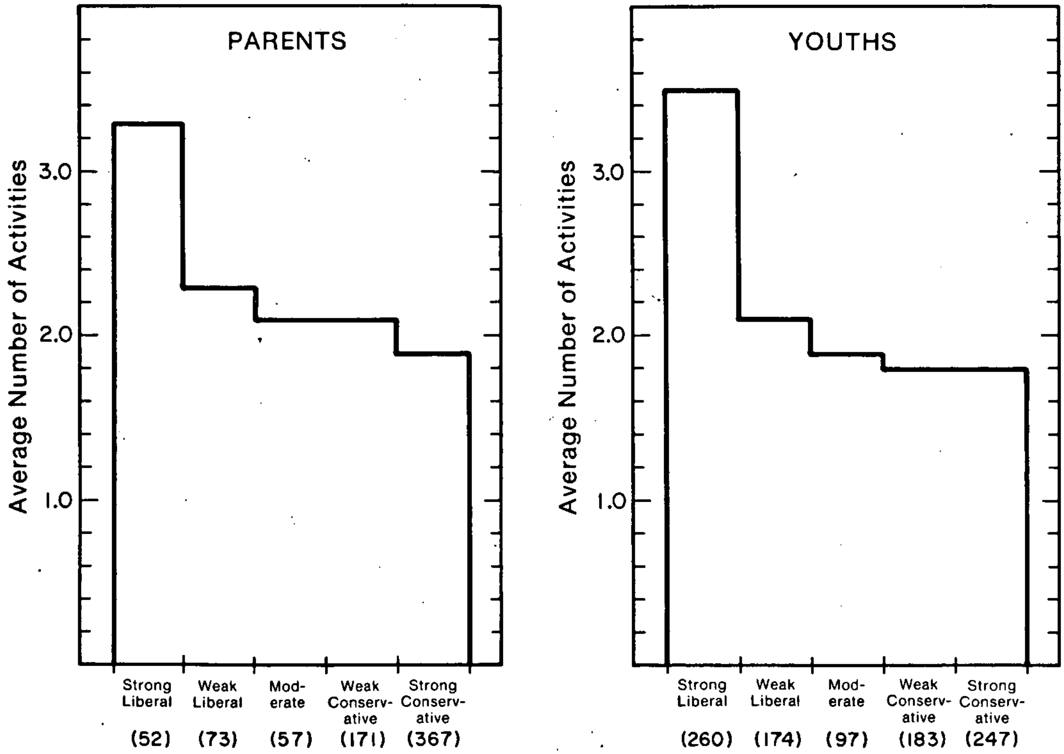
[7] Measuring issue positions at the end of the period of participation assumes, of course, that these positions were held by respondents as they were participating. As tenuous as this assumption might be, it is far preferable to measuring issue positions at the beginning of the period of participation in 1965—the only other time point for which we have data.

The rates of parent and youth participation within each category of the ideology index are presented in Figure 1.[9] The most striking characteristics of these data are the distinct *liberal biases* within *both* groups. If there were no participation bias to ideology, the bars in Figure 1 would rise to uniform heights across the horizon. Instead, there is a spectacular, then



Source: 1973 Parent-Child Socialization Study, Center for Political Studies, University of Michigan.

Figure 1. Participation Bias among Parents and Youths

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[8] Treating the issues in this manner assumes that they fall along a single continuum, which may be labeled liberal/conservative. This assumption is validated by the principal components solution to a factor analysis of the issue correlations, in which all items had substantial positive loadings on the first factor.

[9] Respondents who failed to take a position on one or more of the issues were eliminated from this analysis. Missing data automatically force respondents towards the middle categories of the index. These same respondents tend to have lower rates of participation. The combination of the two factors artificially sharpens the liberal participation advantage in both generations. Even if the missing data are retained, though, the substantive conclusion is the same—the two generations are alike in their liberal bias.

halting decline in political activity as the figure is read from left to right for each group. Activity levels among the strong liberals are almost twice as great as those among strong conservatives. This enormous advantage assumes added significance for the young adults in view of the large size and near-numerical equality of the two most polar groups. Both the disproportionate size of the youthful liberal contingent and its heightened participation have created the impression (which now seems very realistic) of a leftward bias in the political activity of youth during the late 1960s and early 1970s. Although parents exhibit an ideology-participation relationship which is virtually a carbon copy of that for their children, their ideological distribution is radically different. The much higher participation rates of parent liberals are counterbalanced by the enormously larger number of parent conservatives. In this contrast to the filial generation, purely a compositional one, may lie the prevailing view of a generation "gap" in the bias of participation.

These data suggest another, perhaps more surprising, deviation from expected patterns of participation. In the late 1960s and early 1970s, not only did the young participate more than their parents, but both groups also exhibited a liberal participation bias. Later we shall propose that this behavior was in response to the opening up of political alternatives on the left during the period under examination, especially in the 1972 general election. Whatever its cause, though, the remarkable finding here is that both parents and youth deviated from the expected conservative bias in patterns of participation.[10]

Participation and Bias in Broader Perspective

Analysis of these youths and their parents uncovered generational differences in rates of participation but similarities in liberal participation biases. That the findings in both cases were the opposite of those reported in research on earlier periods fueled our expectations that pervasive period effects were at work in the late 1960s and early 1970s. But for all of their considerable advantages, the youth-parent data have limited ability to answer the broader questions we have raised about the unique effects of the period. Because they reflect only two slices of the age-arrayed population distribution, they cannot provide a complete picture of the relationship between age and participation during this period. More critically, because they are time-bound, they cannot confirm our speculations about how the relationships we found differ from those of other periods. We must move beyond this initial data set for a broader perspective.

Evidence from the presidential election series of the Survey Research Center/Center for Political Studies provides this larger canvas. We first used these election series data to determine the relationship between age and participation in each presidential election year in the 1956–1976 period. We gauged participation by an additive index of campaign-oriented activities, using the same campaign activities contained in the overall index of participation in the two-generation analysis: attempting to influence voters, attending meetings and rallies, donating money to campaigns, displaying campaign buttons or stickers, and engaging in other campaign-related work.[11] Respondents in each year were separated into five age cohorts: 21–30, 31–40, 41–50, 51–65, and over 65. The means for the campaign activity index are displayed in Figure 2, by election year and age cohort within the year.

These data show temporal variations in both the participation rates of the young adults and, more generally, in the relationship between age and participation. Of singular interest, young adult campaign activity peaked in 1972, rising well above the rates for the same age group in previous years as well as the succeeding year, 1976. So active were the young in 1972 that their average activity rate fell just short of the highest levels attained by *any* age group throughout the entire series. This finding lends considerable support to the prevailing image of

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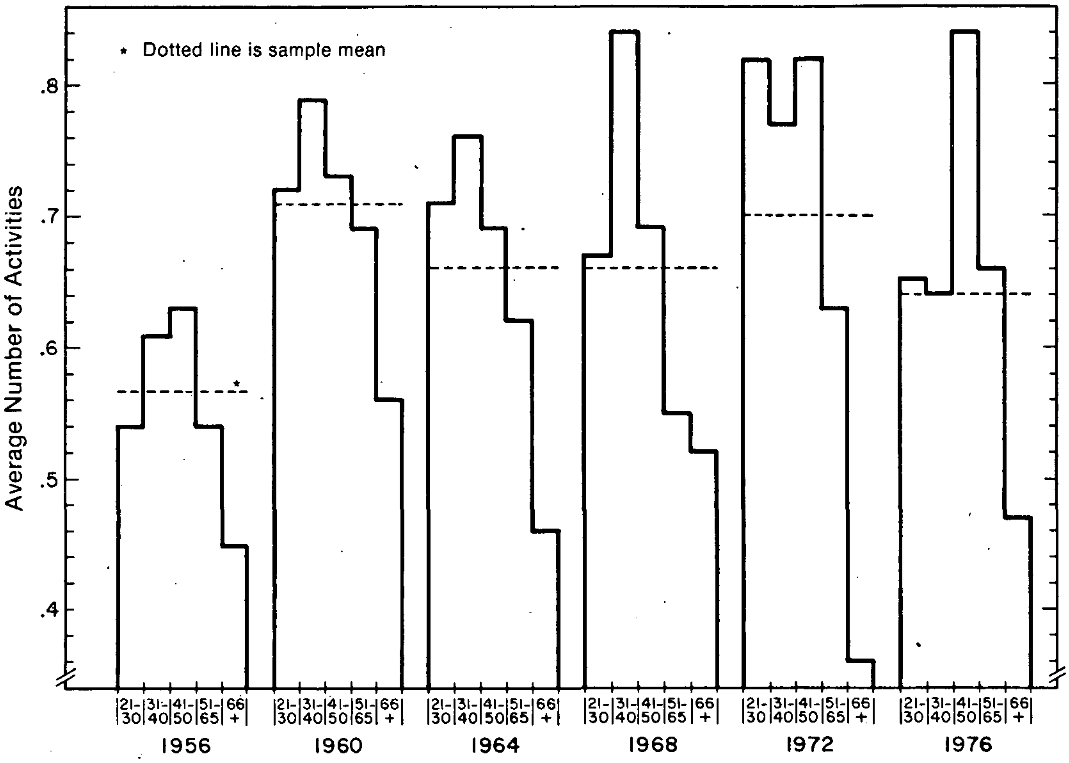
[10] These empirical findings stand up well when alternative measures of ideology are employed. We examined the relationship between ideology and participation for each issue separately and for self-placement on an explicit liberal-conservative continuum. For the youths, the liberals participated more in every case, usually by a very wide margin. For the parents, the liberals were more active in the cases of ideological self-placement and five of the seven issues. In the parental exceptions to the general rule, moderates showed the highest activity levels and liberals continued to outperform conservatives.

[11] Summarizing campaign activity by use of a single cumulative index is methodologically justifiable in each year because each item is correlated substantially and positively with the initial factor in the principal components analysis. Data from 1952 were not used because the question on displaying campaign buttons or stickers was not asked.



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

*Dotted line is sample mean.

Figure 2. Campaign Activity by Age, 1956–1976

exceptional youthful participation around the turn of the decade, especially so for 1972. It is a fair surmise that the youth activity rate in 1972 would appear even more enlarged, and the 1968 rate correspondingly larger too, if protest activities were included. Using the benchmark of similar age cohorts in earlier and later years, these results support our earlier ones in depicting an exceptionally active group of young adults in the early seventies.

Just as striking is the fate of the general relationship between age and participation in 1972 compared with other years. In each election year prior to 1972, a parabolic relationship between age and participation appeared. In three of the years the 31–40 age group occupied the apex of the parabola; in the remaining year, the 41–50 age group held this position. No matter which of these was the peak group, the distribution assumed the neat unimodal form reported by Verba and Nie (1972, p. 139) and widely expected to be the constant pattern for the relationship. The 1976 figures depart from this pattern so slightly as to be inconsequential, as activity in the 31–40 group dropped considerably. But the 1972 results stand in dramatic contrast to the other years, disconfirming any notion of an invariant relationship between age and participation. On the crest of their surge in activity for that year, the youngest cohort outstripped every other age cohort in campaign activism. The campaign activity of the 21–30 group was so high, in fact, that it pulled the overall activity average for that year to within reach of the 1960 activity high for the series, in spite of abnormally low rates of involvement by those over 50. Even in comparison with their elders, then, the young were exceptionally active in 1972.

Before leaving Figure 2 behind, we should note one other interesting feature of young adult participation—this time in years other than 1972. While one or both middle-age cohorts (31–40 and 41–50) outparticipated the young in all but 1972, the young were more active than both of the cohorts over 50 in every year but 1976. Perhaps including community activities and the various forms of personalized contacting would reverse this decline in involvement after age 50. We cannot tell from our materials. What our data do suggest, however, is that one should exercise considerable caution in depicting the twenties as a low participation stage of the life cycle. Even in so-called normal times, Americans in this age group participate more than those over 50. And, in an abnormal time, as the 1972 election period appears to be, the young may come to outparticipate those in every other age group.

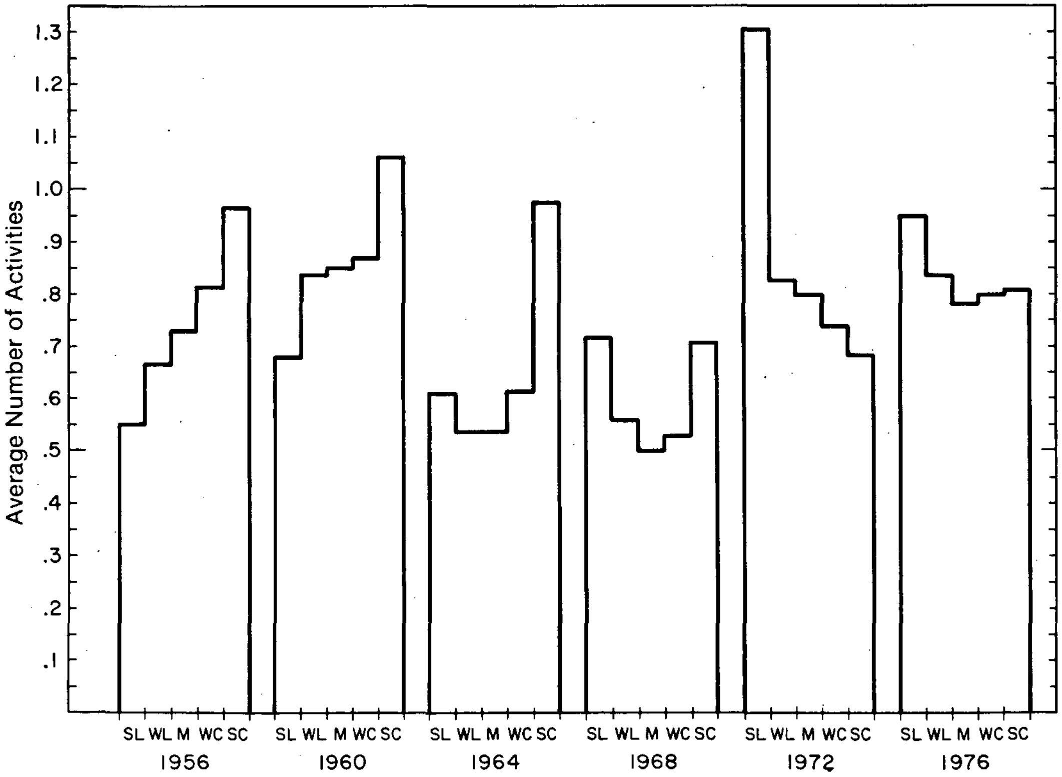
Having employed the longitudinal materials to expand upon our initial findings about participation rates, we now employ them to explore the equally provocative findings about the disproportionately greater participation of liberals in the late 1960s and early 1970s. Because the two generations in the youth-parent study exhibited similar bias, explanations need to focus on factors common to both generations instead of factors which differentiate them. Our hypothesis is that the principal source of this pervasive bias among liberals lies in the nature of the stimuli for political action during the period. While partially a function of the general disposition to participate, political activity is also surely responsive to the opportunities for participation. During the late 1960s and early 1970s, these opportunities were especially widespread for those on the left.

To a great extent the events leading up to and surrounding the presidential elections come to generate and symbolize the divisive forces and strong movements at work in the polity and to characterize the opportunity structure for action. Consequently, it would be wise to look for changes according to ideology precisely in the fluctuating nature of election campaigns. The nature of the candidates in the general election campaigns, where the bulk of the activity is concentrated, should be especially important in determining participation opportunities. Just as the novel and fervent stimulus was on the right in 1964 in the form of the Goldwater movement, it seems to have been on the left in 1968—with the McCarthy and Kennedy candidacies—and even more so in 1972, via the McGovern candidacy and nomination. Of course 1968 also witnessed a strong challenge from the right in the form of the Wallace candidacy, which was channelled outside as well as inside the Democratic party. The Wallace candidacy may have helped to mute the liberal advantage in participation in 1968.

The linkage of the bias of participation to the nature of ideologically based opportunities might help reconcile the liberal bias we have found for the 1965–1973 period with the conservative bias found by Verba and Nie (1972) for the immediately preceding period. The youth and parent respondents were asked to specify their involvement in campaign activities during the 1965–1973 period. This span encompassed two long presidential campaigns, 1968 and 1972, which were characterized by unusually attractive ideological alternatives on the left. Furthermore, the 1972 general election campaign was unprecedented in modern times in that a major party nominee was from the left. Two of the four campaign activities measured by Verba and Nie, on the other hand, were (given the time frame of the questions used) restricted to the 1964 election period, when the opportunities for ideologically inspired involvement lay more on the right. If our supposition about the mobilizing effect of the election period is correct, moreover, then other activities of a non-campaign nature may also have been more attractive to the right at this time.

Analysis of the election series can show whether the relationship between ideology and participation varied over the 1956–1976 period, a necessary condition for the existence of the kind of period effects posited here. As in the case of the age cohort comparisons across time, the indicator of political participation is the campaign activity index generated for each study year. We used opinions on domestic issues of the day as the basis for constructing an ideology index in much the same fashion as for the parental and young adult samples. The index score for ideology was the number of liberal positions minus the number of conservative positions on a series of domestic issues, all of which were substantially correlated with the principal component of a factor analysis. These scores were divided into five categories for presentation: strong liberal (+3 and above), weak liberal (+2 to +1), moderate (0), weak conservative (−1 to −2), and strong conservative (−3 and below). In the 1956–1968 analysis, six issues were included. They dealt with the role of the federal government in the following areas: school integration, fair employment practices, education, medical care, guaranteed jobs, and housing integration (measured with fair employment practices in 1956). Support for government action was coded as liberal, opposition as conservative. The ideology index was comprised of eight issues in 1972. School integration, fair employment practices, and guaranteed jobs were repeated from before. The new issues were legalization of marijuana, women's rights, rights of the accused, government aid for minorities, and busing. Support for each of these policies (1–3) was coded as liberal, opposition (5–7) as conservative, while the middle position (4) was coded as moderate. In 1976, changes in taxes (coded like the new issues in 1972) was substituted for fair employment practices from the 1972 list.[12] Despite the narrower definition of political participation employed, an examination of participation rate by ideological groupings through time should go a long way toward either supporting or impairing the explanation we have proposed for the political activity bias found in the youth and parent generations.

Figure 3 presents the mean activity levels for the five ideological categories across the election series. From left to right in each year are strong liberals (SL), weak liberals (WL), moderates (M), weak conservatives (WC), and strong conservatives (SC). The results provide dramatic support for our proposition that differential bias arises in part from the nature of the participation opportunities of the period.[13] In the elections of 1956, 1960, and 1964 the conservatives participated far more actively



Source: Presidential Election Series, Center for Political Studies, University of Michigan.

Figure 3. Campaign Activity by Political Ideology, 1956–1976

than the liberals. Although only 1956 reveals an almost perfect stepladder pattern, the marked tendency was for activity rates to rise with increasing conservatism. This pattern fits a view of American politics in which a vociferous conservative minority are seen as drowning out the more numerous liberals and middle-of-the-roaders. Indeed, failure to hold this view led the Goldwater forces in 1964 to overestimate drastically the breadth of their electoral support (Converse, Clausen, and Miller, 1965). Significantly, the 1964 results reported here echo the bias reported by Verba and Nie (1972), whose measures of electoral activities in particular were drawn from the 1964 era.

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[12] All other responses to the questions in each of the surveys were coded as missing data, and respondents with any missing data were eliminated from analysis. This elimination lowered the number of cases substantially in several years but did not affect our substantive conclusions. In fact, the effects of eliminating missing data here parallel closely those reported in footnote 9 for the parent and youth data: adding respondents with missing data dampens the political activity rates of the moderates without having much impact on the extremes.

[13] The aggregation of particular issues into an index had little effect on our findings. The pattern displayed using the index was typically replicated (albeit with less clarity) in the patterns of pair-wise association between political activity and the issue positions taken individually.

By 1968 the regularity had been broken. Strong liberals and strong conservatives took part at approximately equal rates, as did weak liberals and weak conservatives at lower levels; moderates were least active. Although this U-shaped distribution fits the classic prescription of a direct relationship between ideological fervor and manifest behavior, it occurs but once across the six elections. If the 1968 results cast serious doubt on the model of invariant relationships between ideology and activity, those for 1972 effectively lay the model to rest. Here the pattern is a mirror image, in slightly exaggerated form, of the 1956 stepladder. Beginning with a tremendously high level of liberal activity, every decrease in liberalism brings a decrease in activity.[14] As of 1976 the strong liberals still held an edge over strong conservatives.

These findings make it clear that ideological bias in campaign activity is variable. Conservative in the 1956–1964 period, the bias shifted to liberal from 1968 to 1976. Which of these periods is the more "normal" cannot be determined with any certainty and is the subject of spirited debate among students of voting behavior (Nie, Verba, and Petrocik, 1976, pp. 110–55; Converse, 1976, pp. 98–107). However this debate is resolved, we have established that the relationship between ideology and participation varies over time, presumably dependent in part upon the nature and attractiveness of the ideological alternatives presented to the mass public. Within this framework, the bias displayed by the parental and filial generations in our earlier analysis acquires an almost inevitable shape. Indeed, the comparison of the activity means for the ideological groupings shown in Figure 1 bears remarkable similarity to that for the national cross-section sample of 1972 in Figure 3, even though our two-generation samples are biased upwards in educational attainment. Both parents and youth in our initial study were reacting to the broad sweep of political forces in the same manner as was the adult population as a whole. Further, as the data in the appendix show, these findings are unchanged when the Verba-Nie technique for expressing bias is used.

This changing relationship between ideology and participation over time may well contain the key to the unexpectedly high relative rates of participation by the young in the early 1970s. For the first time in the electoral series, the ideological position of the young diverged markedly from that of older adults, as there were almost twice as many young among the strong liberals. Indeed, the 1972 ratio of youthful to older strong liberals *itself* was virtually twice what it had been in any earlier year. This disproportionate youthful propensity for strong liberalism persisted into 1976, although at a level slightly below that recorded in 1972. While liberals outparticipated conservatives again, the gap had narrowed considerably and youthful activity had declined. Perhaps the absence of a candidate on the left led to both changes. As we saw with the two-generation data, uneven rates of mobilization from different sectors of the ideological continuum produced greater rates of participation by the young in the early seventies, but only because there were proportionally more of them on the left. It appears that the youthful edge in participation at this time can be traced to the fact that opportunities for left-oriented participation came at a time of disproportionate youth liberalism—a much more complex compositional effect than we entertained earlier.[15]

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[14] A parallel change in the relationship between participation and ideology in 1972 is noted by Nie, Verba, and Petrocik (1976, pp. 207–09), although they do not analyze or interpret it as fully as we have. Among both Republicans and Democrats, they found that ideologues participated more in that year. In previous years, by contrast, ideologues participated noticeably more only among Republicans.

[15] Direct comparisons of the ideological distributions across the different years should be avoided. Even where the same issues are used to define ideology, changes in the meaning of the issue and in the format of the question undermine strict equivalence (Sullivan, Piereson, and Marcus, 1978; and Bishop, Tuchfarber, and Oldendick, 1978). We have not made much of the changing distributions along the ideology continuum for this reason, restricting our attention instead to group differences within an election year and how these *differences* varied over the years.

Conclusion

Analysis of two complementary data sets, the youth-parent cohorts and the 1956–1976 presidential election series, demonstrates that young adults in the late 1960s and early 1970s were indeed exceptional in their political participation. The young participated at higher rates than did the young in other years, and they were more active than mature adults during this period. Both patterns were unusual and unexpected. But the exceptionality of the young was confined to their rates of participation. Where ideological bias is involved, young liberals were joined by older liberals in being more active than their age cohorts who occupied more conservative positions along the ideological continuum—again an unexpected and perhaps exceptional pattern.

Our findings suggest that substantial revisions are in order for two aspects of participation theory. In the first place, there is no constant relationship between age and political participation. In certain periods, the neat pattern of increasing participation rates until about age 65 is disrupted. The late 1960s and early 1970s were one such period, as the youngest members of the electorate came to outparticipate their elders. Nor can one account for these findings by introducing standard compositional variables. Although the educational attainment of same-age cohorts was rising over time, participation was fluctuating. More to the point, radical discontinuities in the shape of the relationships, such as in the 1968–1972–1976 series of elections, could not possibly be accounted for by demographics alone.

In the second place, there is no immutable tendency for conservatives to be more politically active than either moderates or liberals. Again, in certain periods, the expected conservative edge is overcome. For example, in the early 1970s, liberals became the most vigorous activists among both the young and other age groups.

These findings show that political participation does not exist in a political vacuum. It is commonly, some might say always, a mass response to elite activities (Miller and Levitin, 1976). This can be demonstrated most easily for campaign-related activities, but it probably extends to other activities as well. One's involvement in a political campaign may be seen as a function of, inter alia, the attractiveness of the serious candidates in that campaign. It is surely no accident that the largest participation advantage of strong conservatives came in the Goldwater era. By the same token, a Democratic primary, such as that in 1968 or 1972, with many candidates on the left, offers more inducements for leftists to participate than does a campaign in which there are no candidates from the left. Even greater opportunities to participate arise in the context of the general election campaign. Thus, it should come as no surprise to find that left-wing participation peaked in 1972, just as right-wing participation was relatively highest in 1964. Similarly, a general election campaign in which two centrists are the combatants (such as 1976) probably curtails the incentives for left- and right-oriented involvement and decreases the ideological biases in participation.

The period beginning around the mid-sixties and extending into the early seventies saw a decisive change in the battleground of American politics. Challenges to the prevailing liberal consensus were launched primarily from the left, with the result that the consensus and perhaps even liberalism itself were shattered. The first challenges came over civil rights and the war in Vietnam, but the focus quickly spread to matters of urban decay and non-black minority rights. Serious candidates championing these causes emerged especially in contests for the Democratic party nomination in both 1968 and 1972, and success in these preliminary rounds enabled one candidate to carry his party's banner in the presidential election. Opportunities for protest participation were likewise expanded on the left. One has to go back at least to the 1930s in American political history to find comparable stimulation for left-oriented participation.

The findings presented here show convincingly that these opportunities were seized by people whose operational ideologies placed them on the left. In this period leftists attained levels of participation far in excess of those of their peers elsewhere along the ideological continuum—in contrast to what was thought to be normal, right-biased patterns of participation. By charging the left pole of the opportunity magnet, in other words, participation on the left was made relatively more common. But left-oriented Americans were not distributed evenly among all age groups. Instead, ideology was heavily skewed by age, with the familiar result that the young were much more left-oriented than older adults, including their parents. It seems quite likely that the unusual youthful edge in participation during the late 1960s and early 1970s was the result of these greater openings on the left. While the opportunities may have been seized at roughly equal rates by young and old, the distinctively youthful cast to the composition of left-wing Americans pulled proportionally more of the young into participation. Thus, the potential for a disruption in the "normal" tendency for lower participation among the young exists whenever the ideological postures of the young diverge from those of the remainder of the population.

The surprising patterns of participation for the late 1960s and early 1970s presented in this study seem far removed from current, quieter times. As of 1976, the young adult advantage had vanished, and the liberal bias had narrowed considerably. That these advantages emerged during one period of American politics, however, illustrates how the considerable slack in the use of participation resources can be used to benefit different groups at different times. Propositions positing static relationships between either age or ideology and participation are misleadingly incomplete. Far more useful is an approach which can handle "deviance" as well as "normality" by adding an explicit political factor, the opportunities for involvement, into the participation equation.

Appendix: An Alternative Measure of Bias

Verba and Nie (1972, Ch. 15) employ exactly the opposite method from ours for expressing the relationship between participation and ideological position. They array the ideology averages by each level of political activity. This procedure is well suited to their goal of distinguishing activist preferences, which are most likely to be perceived by leaders, from non-activist preferences. Our purpose is different, to show whether people with one ideological persuasion participate more than those with another. In this respect, our analysis parallels the brief considerations in the latter portions of Verba and Nie's chapter 12 and again in Nie, Verba, and Petrocik (1976, pp. 206–09). These two procedures are, in effect, opposite ways of analyzing the same data matrix. As such, their results are not entirely independent of one another.

We have run the analysis both ways to demonstrate that our findings are not merely an artifact of the way we chose to "percentage the table." The results from the Verba and Nie procedure applied to our data are presented in Table 2. The cell entries are the ideology means for each activity category; the higher the ideology mean, the more liberal are the respondents at that activity level. Comparing these results with those from Figure 3, it is clear that essentially the same substantive conclusions are in order regardless of which variable is spread against which variable. Contrary to patterns in earlier years, liberals are more active (as we showed in Figure 3) *and* activists are more liberal in 1972 and again, though less uniformly and less sharply, in 1976. As in Figure 3, inter-year comparisons in liberalism scores are perilous due to the lack of equivalence in measures.

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Table 2. Mean Liberalism by Activity Level

	Number of Activities Performed					
Year	0	1	2	3	4	5
1956	2.3	2.0	1.2	1.1	1.2	0.6
1960	2.5	1.6	1.6	1.4	1.2	1.8
1964	0.4	0.3	0.0	−1.1	0.1	−2.1
1968	0.2	0.2	0.7	0.4	0.6	−0.6
1972	−0.7	−0.3	0.3	2.0	2.2	4.1
1976	−1.3	−1.3	−0.7	−0.6	−0.5	−0.4

Source: Presidential Election Series, Center for Political Studies, University of Michigan.

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Ideological Interpretations of Presidential Elections

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This article presents a new way to define and measure the ideological sentiments of the mass electorate. Citizens are classified in terms of their evaluations and perceptions of liberals and conservatives. The measure is then used to assess the impact of ideology on the 1972 and 1976 presidential elections, to explore citizens' applications of ideological labels to parties, issues, and presidential candidates, and to describe the relationship between ideology and the potential for party realignment as well as meanings of issue voting.

Although many Americans use ideological labels in ways that suggest only a partial understanding of the terms and their implications, those labels have political significance for their political attitudes and election-day decisions.

Two decades of research on political ideology in mass electorates have produced two general conclusions and a host of controversies. The first conclusion, well summarized and persuasively argued by Converse (1975), by Klingemann (1979, forthcoming), and by Stimson (1976), is that only small minorities, perhaps 15–20 percent of the electorate in the United States, Austria, Great Britain, France, Germany, Italy and the Netherlands, exhibit even the rudimentary attributes of the political ideologue.[1] The second conclusion is that people often use ideological symbols or labels with little comprehension of their ideological meaning, even though 70 percent or more of the citizens in these mass electorates may use them to describe the political parties (Miller and Miller, 1976, p. 844; Klingemann, 1979, forthcoming). This article classifies members of the electorate in terms of their ideological understanding of politics, and argues that the use of ideological labels to describe political objects and events, as well as the parties, the issues and the candidates involved in presidential elections, plays an important role in voters' behavior (Edelman, 1964). We shall demonstrate that examining the popular use of the ideological labels *liberal* and *conservative* provides important insights into the nature of mass electorate behavior, and that the "nonideological" use of ideological labels is an important feature of mass politics in America.[2]

The major controversies in the study of ideology in mass societies have focused on assessments of the prevalence and content of ideological thinking among rank and file citizens. One controversy has concerned the appropriateness of the criteria and the methods used to define and measure the prevalence of ideological thought. Stimson's (1976) discussion of some of the differences in perspective followed the earlier extended discussion of issue voting in the June 1972 *American Political Science Review* and anticipated some of the argument in the subsequent *Review* symposium (Miller et al., 1976; see also Converse, Clausen and Miller, 1965, pp. 321–36; Miller and Levitin, 1977, pp. 171–74). This article will neither address that controversy nor extend the discussion of whether, as a result of the methods used to define and measure ideological thought, the "ideological glass" is nearly empty or almost full. Rather we propose to assess the use of the terms *liberal* and *conservative* as they are applied by citizens to describe themselves, the political parties, presidential candidates, and positions on issues of public policy. Thus we will not present a content analysis of verbal responses to open-ended questions, a strategy followed by both Converse (1975) and Klingemann (1972).[3] In our own earlier work we adopted the more common strategy of assessing intercorrelations among issue positions to test for the presence of ideological proclivities. However, this often ambiguous mode of exploring the consequences of ideological thinking, the basis for the work of Nie and Andersen, is not central to the present analysis (Miller and Levitin, 1977, pp. 171–74).[4] Instead, our principal measurements are based on citizens' direct application of the liberal and conservative labels to themselves and to political objects. This choice is not intended to by-pass or settle the controversies over method; it is intended to provide a new focus on the role of ideological labels in American mass electoral politics.

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[1] See Klingemann (1979, forthcoming) for a new set of estimates for Britain, the United States, Germany, Austria and the Netherlands; see also Barnes (1971); Butler and Stokes (1969).

[2] The data used in this analysis were originally collected by the Center for Political Studies of the University of Michigan under NSF grant numbers GS–33956 and SOC76–13562. The authors are indebted to Celinda Lake for her research assistance and to Philip E. Converse for his comments on an early draft. We also wish to thank Nancy Brennan, Josephine Lopata, Joyce Meyer, Laura San Facon, and Judy Siegfried for their assistance in the preparation of this article. The data are available through the Inter-University Consortium for Political and Social Research.

We focus on the ways in which citizens use ideological labels to understand the world of politics. The first section describes our measure of ideology and examines the distribution of people, and changes in this distribution, as they classify themselves on a liberal/conservative continuum. Next, we explore the relationship between ideological location, or self-placement, and vote preferences to begin to delineate the role of ideology in mass electoral politics. We attribute changes in the presidential vote between 1972 and 1976 to perceptions that are related, in turn, to ideological self-classification and to party identification. Our next two sections examine citizens' applications of the ideological labels *liberal* or *conservative* to parties, issues, and presidential candidates. These sections demonstrate the extent to which the use of ideological labels has permeated American politics and also suggest how ideological labeling may contribute to the absence of party realignment. Finally, we present a detailed inspection of the linkage between issue positions and liberal/conservative distinctions to describe how American citizens use—and misuse—ideological labels to describe their policy positions.

Our analyses lead to two major conclusions: (1) many Americans use ideological labels in ways that suggest only a partial understanding of the terms "liberal" and "conservative" and of their implications, and (2) those labels nevertheless have political significance for these citizens, just as they do for more sophisticated voters. It has been demonstrated that a primitive and unelaborated sense of party identification links some persons to a political process from which they would otherwise be separate. We think that ideological labels may create links that help many citizens make sense out of the remote world of politics, even though they attach little systematic content to those labels. A network of associations between the ideological labels *liberal* and *conservative* and political objects may provide political anchoring. Though relatively devoid of articulable meaning, these labels may provide many people with a basis for a fuller understanding of the political world.[5]

Measuring Ideology in Terms of Liberal/Conservative Self-Labeling and Evaluation

In both 1972 and 1976 respondents in the

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[3] For many purposes the direct coding of such material doubtless constitutes the most valid of alternative methods, particularly if the questions used to elicit the verbal responses are structured and worded to avoid coding distinctions that reflect no more than the difference between respondents' active and passive vocabularies. The early levels of conceptualization measure is open to questions not appropriate to subsequent work of Converse and Klingemann in that persons for whom relevant abstractions were a well-understood part of their *passive* vocabulary may have been mis-classified by level of conceptualization because the abstractions were simply not used as people described what they liked or disliked about the parties or the candidates. The problem may be analogous to that encountered by studies of mass response to congressional candidates. Much of the literature documenting voters' lack of knowledge about congressional candidates is based on questions that demand recall of the candidates' names as the first criterion of knowledge. If, however, the voter is simply asked about *named* candidates, the measure of voters' knowledge increases rather dramatically. In any event, where open questions ask directly what terms such as *liberal* and *conservative* or *left* and *right* mean, these problems are obviously avoided.

[4] It should also be noted that the interpretation of correlations among issue positions is not always straightforward and, indeed, one of the current controversies centers on that fact. See Nie (1974); Bishop et al. (1978); and Sullivan et al. (1978).

[5] Klingemann has suggested to us that in Europe it is likely that many citizens with only limited knowledge of the politics of neighboring countries may well make sense out of the international news of politics largely through using the left-right labels to link the vaguely familiar—their own parties—with the unknown entities abroad. If, as he further suggests, the non-ideologue understands left-right labels largely in affective terms—you can't trust the left, people on the right are kind—the linkages may transfer the affect and produce political reactions that are as important as those based on much more elaborate "ideological" views. The recent elections for the European parliament may provide an interesting test of the existence and the consequences of such linkages.

Center for Political Studies surveys were asked directly about their own associations with the terms *liberal* and *conservative* in three different ways. First, respondents were asked to locate themselves on a seven-point scale bounded by "people whose political views" were either liberal or conservative. Later in the interviews respondents were presented with a list of groups and asked to identify the groups "that [they] feel close to—people who are most like [them] in their ideas, interests and feelings about things." Liberals and conservatives were among the groups on the list. Finally, still later in the interviews, respondents rated liberals and conservatives on the "feeling thermometer."

We have combined responses to these three items to form a measure of ideological self-placement. This measure combines both cognitive and affective responses, both opinions and feelings, in order to produce a global assessment of the degree to which respondents use these labels as reference points to locate themselves in the political world. Specifically, respondents who located themselves as liberal (or conservative) on the initial seven-point scale by choosing scale positions 1, 2, 3 (or 5, 6, 7) and who subsequently said they felt close to liberals (or conservatives) were classified at the extremes of the final distribution as Ideological Liberals (or Conservatives). The two next and less extreme categories were composed of those who located their own political views as liberal (or conservative) on the seven-point scale (selecting positions 1–3 or 5–7, respectively) but who did not feel close to either ideological group. These respondents were simply classified as Liberals (or Conservatives). Those respondents who said they were in the middle, neither liberal nor conservative (point 4 on the seven-point scale), not saying they felt closer to liberals or conservatives, were classified as centrists. However, the thermometer measure was used to divide the centrists into three categories. If a respondent rated either liberals or conservatives at 60° or higher, and if that rating was at least 10° higher than the rating for the other group, the respondent was classified as a centrist with either a Liberal or Conservative Preference. Those who initially located themselves precisely in the middle of the continuum and who showed no affinity for either liberals or conservatives on either the "closeness" or thermometer measures were labeled Centrists. We maintained a residential category of those who initially said they had not previously thought of where they would locate themselves on the liberal/conservative continuum, or who gave contradictory responses in the question sequence just described.

This strategy produced a measure that combined all of the direct assessments of responses into a global rating that allows us to differentiate degrees of ideological self-placement within the mass electorate. This strategy also produced a measure that closely parallels the traditional measure of party identification. Our residual category of those not located is a reasonable match for the Apolitical category of those who do not think of themselves as Republicans, Independents or Democrats; the middle category is the ideological analogue to the classification of the Independent–Independents who reject any partisan labeling; the categories adjoining the center are based on preferences similar to those of the Independent Leaners; and the end-points of our new measure where the most ideological of the electorate locate themselves are analogous to those occupied by the persons who describe themselves as strong party identifiers. As is the case with party identification, this scale of ideological location is based on responses to structured but open-ended questions, rather than forced-choice questions, and is, therefore, based on individuals' own descriptions of their locations, affinities, and preferences. Table 1 presents the national distribution of the electorate on this measure for the last two presidential elections.

The table indicates that, in sharp contrast to the limited incidence of apolitical persons on the party identification measure where only one or two percent fall into that category, about one-third of the national electorate describe themselves as not having thought about how to locate themselves as liberals or conservatives.[6] Another one in ten placed themselves in the center with no preference for either ideological alternative in 1972 or 1976. The remaining 60 percent did describe themselves in terms of some degree of ideological preference. Thus, well over half the electorate can, by this measure, be said to think of themselves as expressing some ideological sentiment or having some ideological location.

The table also indicates that conservatives outnumbered liberals by a margin of almost 2 to 1 in both years. The preponderance of

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[6] The persons classified as "Not Located" include 2 or 3 percent with inconsistent response patterns, e.g., self-placed liberals who felt closer to conservatives, etc. The problems of measurement that result from the use of the verbal stimuli "Republican, Democrat or Independent" for party identification in contrast to the labeled seven-point scale for liberal/conservative location are being explored in methodological work preparing for the 1980 CPS/National Election Study.

Table 1. Ideological Location of the Electorate, 1972 and 1976

	1972	1976
Ideological Liberal	7%	7%
Liberal	10	9
Liberal preference	5	4
Centrist	10	12
Conservative preference	12	9
Conservative	16	13
Ideological Conservative	10	12
Not Located	30	34
Total	100%	100%
N =	2147	2355

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

self-proclaimed conservatives preceded any post-1976 swing to the right, if such has occurred.[7] The lack of any substantial indication of such movement in the post-1972 period calls into question the presumption that the reelection of Nixon and Agnew and the virtual disappearance of agitation from the New Left marked the beginning of a mass reaction to and rejection of the policies and symbols of the Left. Furthermore, the distribution of the preferences is quite stable across the four-year interval separating the two independent derivations of the measure. However, it is true in this instance, as in many others, that aggregate stability conceals substantial individual instability and change.

Stability of Ideological Location Across Two Presidential Elections

The aggregate stability for the nation between 1972 and 1976 was the product of countervailing changes that cancelled each other. Specifically, a sense of a national move to the right might have been produced by the conservative swing among the most visible of citizens, those who participate most actively at election time. Indeed, much of the popular press suggested that the electorate had moved to the right in that period and was continuing to do so following the election of 1976. At the same time, the least active, articulate and visible (including the nonvoters) actually moved to the left in sufficient numbers to offset the changes among the more visible and active citizens.[8]

Table 2. Ideological Location in Relation to Levels of Political Participation, 1972 and 1976

	Participation					
	Active Participants		Voted Only		Nonvoters	
Ideological Location	1972	1976	1972	1976	1972	1976
Ideological Liberal	10%	11%	9%	6%	2%	3%
Liberal	11	10	9	9	9	6
Liberal Preference	1	6	5	5	4	1
Centrist	9	9	9	11	7	14
Conservative Preference	17	10	13	10	10	7
Conservative	16	14	16	14	14	13
Ideological Conservative	14	21	14	14	3	1
Not Located	21	20	25	30	52	55
Total	100%*	100%	100%	100%	100%	100%

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Because of rounding to integers, some columns do not add to 100 percent.

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[7] An index of sentiments toward liberals and conservatives, based on thermometer ratings of the groups from 1964 to 1976 (percent favoring liberals/percent favoring conservatives) reflects the following consistently pro-conservative balance over the past seven elections:

1964	1966	1968	1970	1972	1974	1976
−8	−14	−15	−20	−19	−14	−17

The persistence of the same magnitude of preference for conservatives over liberals contrasts with the spate of semi-popular and professional writing on the general theme of a post-1972 shift to the right in American political thinking.

[8] A similar hypothetical situation was suggested in Campbell's *The American Voter* (1960, pp. 256–65) in a discussion of "differences in conceptualization and the interpretation of partisan change." Also see Converse (1965) for a discussion of the distinctive characteristics of those who are politically active.

Table 2 presents the distributions of the ideological locations of citizens when they are sorted by their degree of participation in each of the last two presidential elections. The measure of participation that is used is the familiar separation of (1) nonvoters, (2) voters who did not participate beyond going to the polls, and (3) voters who also engaged in some other activity—predominantly consisting of talking to others and trying to persuade them to support their own party or candidate.

The distributions of all three participation groupings show some change between 1972 and 1976. However, as might be expected from the relative reduction in the number of cases alone, the smaller groups—the activists and the nonparticipants, each containing about 20 percent of the electorate—show more "wobble" suggesting individual change than does the much larger middle group of voters who make up almost 60 percent of the electorate. Moreover, among the nonvoters, the proportion showing no ideological preference—the centrists *and* those not located on the measure—increased by a full 10 percentage points between 1972 and 1976. There was, however, no similar change at all among the active participants. Within the nonvoter and voted-only groups, the net result of the changes suggests no large shift in the liberal-to-conservative ratio, and certainly no major shift to the left. Among the active participants, however, there is some suggestion of a shift in the net balance; but it was a shift to the left, a shift that increased the number of liberals and decreased the number of conservatives. At the same time, the growth of liberal strength among active participants was entirely among those with Liberal Preferences; on the conservative side, although larger losses occurred among the conservatives and those with only a Conservative Preference, the number of Ideological Conservatives actually increased by a full 50 percent (from 14 to 21 percent of all active participants). Within the ranks of the political activists, therefore, the balance between the more extreme groups of Ideological Liberals and Ideological Conservatives thus shifted from less than 3 to 2 conservative over liberal in 1972 to almost 2 to 1 conservative over liberal in 1976. This latter change could have contributed to a widespread—though mistaken—impression of a more general national swing to the right throughout the electorate. This shift to the right by a very small minority of activists was actually more than offset by a series of smaller shifts to the left among the remainder of the electorate, but the shift leftward occurred among less visible citizens, and was, therefore, overlooked by many politicians and political analysts alike.[9]

Our search for countervailing shifts among politically significant subgroups in the population was extended to groups of party identifiers. Among Democrats and Independents, particularly among the latter, there was a small decline in conservative strength between 1972 and 1976. In both groups this was largely the result of Conservatives and Centrists with Conservative Preferences in 1972 describing themselves only as Centrists or as having no ideological location by 1976. Among Republican identifiers and sympathizers the pattern of change was markedly different. Those who described themselves as any type of conservative increased slightly in *total* numbers, from 60 to 63 percent. However, the largest change was in the proportion of Ideological Conservatives among Republicans; they increased sharply, from 24 percent in 1972 to 32 percent in 1976.

Thus it appears that, although there was no national swing to the right between 1972 and 1976, a swing to the right did occur among Republicans as well as among the most active of the electorate. This documentation of a shift to the right among Republicans is consonant with the strong support given to Ronald Reagan (in 1976) who received more votes than Gerald Ford in the Republican primaries. But politicians and political analysts who concluded that support for Reagan reflected a general move to the right by the entire electorate were incorrect; in fact, such movement was to be found only among Republican partisans.

The analyses of interelection shifts in ideological locations within different levels of political participation and within different categories of party identification provide evidence of some individual-level change and instability of ideological sentiments. However, much of the change in the balance of ideological preferences was concentrated within the more partisan and most politically active groups. The relative concentration of such change in such groups—groups that usually reflect the greatest stability in their attitudes as well as their behavior—suggests that these changes were neither random nor the result of unreliable measurement. When this interpretation of observed change is taken into account, an overall estimate of the stability of ideological location suggests that it may be useful for the continuing analysis of American electoral behavior.

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[9] A very similar problem in elite perceptions and interpretations of change in public opinion was examined in our earlier analysis of changing attitudes toward the politics of the New Liberals in the period 1970 to 1972 (see Miller and Levitin, 1977, pp. 93–118). In that instance, however, the true magnitude of the shift to the left was *under*estimated by most political observers because the shift of the less active was *congruent* with the movement of the more active—away from the right toward the center as active citizens moved from the center to the left. In the current instance, the invisible shift of the inactive more than countered that of the high participants, but the invisibility of their move led to an *over*estimation of the pervasiveness of movement to the right.

A summary of the stability of ideological location at the individual level can be provided by a continuity correlation. This correlation can be calculated across the four-year interval for that subset of respondents who formed the 1972–1976 panel in the CPS election study series. In order to attach some relative meaning to this correlational summary, we also computed the correlation for party identification (one of the constants for electoral analysis) and for each of the policy questions probed with a seven-point scale in both studies (questions that have been used to assess changes in the prevalence of ideological thought).[10] The comparable correlations were .80 for party identification, .65 for ideological location and an average of .46 across policy preferences on nine issues.[11]

The full meaning of the differences in these correlations is not obvious. In the absence of national political organizations or parties of liberal and conservatives, and of national elections institutionally defined as competitions between liberals and conservatives, it is hard to imagine circumstances in the United States under which ideological locations would be as well anchored and as stable as partisan locations. On the other hand, these correlations indicate that ideological location was much more stable than were the several expressions of policy preference which it is sometimes presumed to summarize. Why this should be so is not clear unless, as we shall suggest, ideological location is more than a summary statement of policy preferences. John D. Holm and John P. Robinson (1978) draw just such a conclusion. In any event, the measurements we are using are available only for the two presidential elections being reported here, and we do not know how the ideological character of the national electorate during this brief period compares with that of other eras.

Monitoring changes in individuals' ideological location can help us to understand its role in electoral behavior. In the meantime, we should remember that the importance of the concept and measurement of party identification goes beyond the fact that voters consistently classify themselves in terms of their party loyalties. Rather, the principal value of measuring party identification lies in how it can be used to understand the voting choices, responses to issues, evaluations of candidates, and other political attitudes and behavior of the electorate. So too, the value of conceptualizing and measuring liberal/conservative ideological location as we have proposed will rest on its contribution to understanding the political attitudes and behavior of the electorate. The remainder of this article will focus on some of the relationships between ideological self-labeling and responses to the world of politics.

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[10] Analogous continuity correlations for party identification and issue positions are reported for two-year intervals in Converse and Markus (1979). To increase comparability with their work, particularly with the correlations based on seven-point scales of policy preferences, we have followed identical procedures here in handling "missing data" categories—including the eighth category composed of those who could not be located on the liberal-conservative dimension. However, the four-year continuity correlations reported here are not strictly comparable to the Converse-Markus correlations. Most importantly, their Party Identification correlations (averaging .71) are based on virtually the entire population (excluding only the apoliticals, minor party adherents and the "NA"), while the party, ideological and issue correlations reported in this article are based on only the 60–65 percent who held relevant ideological attitudes at both points in time. When those who "fall out" on the ideology or issue correlations are also excluded from the Party Identification correlations, the Party Identification measure rises to the reported .80. Three different measures have been used in the construction of the ideology measure—although they are not all used on all of the population but are employed only selectively—and the multiple-item base may contribute something to the greater stability of the ideology measure over that of the issue measures.

[11] The continuity correlation for ideological locations goes up to .74 when limited to persons who were high in political participation both in 1972 and in 1976 and it drops to .44 among the habitual nonvoters. Given the volatility with which perceptions, attitudes and political beliefs of nonparticipants appear to change, and given the minimal impact that nonparticipants have on election outcomes, it may be useful in future work to consider a general analytic strategy for studies of electoral behavior that eliminates nonparticipants at the early stages of any analysis that concentrates on partisan choice. As can be seen in Table 2, estimates of meaningful national parameters change with the exclusion of habitual nonvoters who constitute a large group that includes 60–70 percent with no ideological preference at all and no more than 4 to 5 percent who fall in either extreme category of liberal-conservative location.

Ideological Location and the Vote

It is generally agreed that ideological concerns were especially salient in the 1972 election, an election said to be uniquely influenced by issue voting, the presumed expression of these ideological concerns. The election of 1976 was not an issue election, however. Ford's congressional record as a conservative Republican was not a major focus of discussion, and many observers and reporters saw Carter as neither conservative nor liberal, but, instead, as "fuzzy" on major questions of governmental policy. Most analysts agree that in the 1976 election party loyalties and candidate preferences were once more the most important determinants of the vote for substantial proportions of the population, and that ideological concerns and issue voting were of far less consequence than in 1972.

To assess the relative contribution of party identification and political ideology to the vote outcome in both elections we examined the relationship between them and then proceeded to a Normal Vote Analysis of the two years.[12] Table 3 shows how citizens classified themselves in 1976 both as to ideological location and partisanship. The joint distribution of the two measures is very much the same in the two years and so only the data for 1976 are presented here.

As Table 3 indicates, the Democrats were rather evenly divided between liberals and conservatives, and relatively few Democrats across all categories of liberals could be classified as Ideological Liberals. Among Republicans, however, Ideological Conservatives were the modal group, and among strong Republicans, where conservatives outnumbered liberals by 72 to 4, Ideological Conservatives outnumbered other conservatives by 39 to 33. Thus, Republicans were more homogeneous than Democrats in their ideological self-placement, and they were also much more often unqualified in their ideological commitments.[13]

The comparative normal vote analysis presented in Figure 1 permits us to evaluate the direct role of ideological self-placement, apart from partisanship, in determining the actual vote divisions. As can be seen from Figure 1, ideological concerns apparently made a separate and significant contribution to the vote decision in both 1972 and 1976.[14] Although the

Table 3. Party Identification in Relation to Ideological Location, 1976

	Party Identification						
Ideological Location	D_s	D_w	I_d	I_i	I_r	R_w	R_s
Ideological Liberal	11%	11%	14%	3%	2%	1%	1%
Liberal	13	10	14	9	7	4	2
Liberal Preference	5	4	8	5	4	2	1
Centrist	11	15	11	15	10	11	2
Conservative Preference	6	7	8	10	14	13	10
Conservative	10	11	9	9	19	21	23
Ideological Conservative	4	6	7	6	27	20	39
Not Located	40	37	30	42	19	28	23
Total	100%*	100%	100%	100%	100%	100%	100%

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Because of rounding to integers, some columns do not add to 100 percent.

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[12] Since the primacy of candidate evaluations in both years seems clear, we concentrate on trying to disentangle the relative importance of the other two major facets of electoral response (see Miller and Miller, 1976).

[13] Here, as in other bivariate distributions of party identification and the use of ideological labels, the ideological cast of partisan leaners is apparent. Within their respective party groups, the Independent Leaners are more likely to locate themselves on the liberal/conservative continuum than are the strong party identifiers. Moreover, they are more homogeneous in their ideological self-characterization than are the weak identifiers (see Brody, 1977; Miller and Levitin, 1977, pp. 251–53).

[14] The "expected" values are derived using the Normal Vote algorithm developed and first presented in Philip E. Converse (1966). The derivations of the long-term and short-term indices are after Richard W. Boyd (1972a, 1972b).

correlation between partisanship and ideological position did increase slightly by 1976, as reflected in the increase in the long-term index, from 9.5 to 11.5, there was virtually no decline in the independent contribution of liberal-conservative sentiments to the vote decision between the Nixon-McGovern and the Carter-Ford contests.[15] The conventional wisdom about differences in the ideological ambience of the two elections suggests that ideological preferences should have been far more highly associated with the vote in 1972 than in 1976. Yet the impact of ideological sentiments on the vote was virtually the same in both years. Party voting may have been more important to the election outcomes in 1976 than in 1972, and issue voting may have been less important in 1976 than in 1972, but voting in accord with one's ideological sentiments was virtually as prevalent–independent of party voting, as reflected in the short-term indices–in 1976 as in 1972.[16]

This normal vote analysis draws attention to a continuing and significant relationship between ideological location and presidential vote choice in both 1972 and 1976, a continuity that has not been widely noted. The analysis also permits identification of those who changed their vote from Republican to Democrat, thereby describing more precisely the source of continuity and change in voting behavior associated with differences in ideological location. Figure 1 reveals substantial variations in the extent of this change across the liberal-conservative continuum. The fortunes of the Democratic candidate improved by almost the same margin among Ideological Conservatives as among Ideological Conservatives between 1972 and 1976. Larger increases in the Democratic vote were located within all but one of the other five categories of ideological location.

Once again, however, conclusions appropriate to large groups of the population may be modified by analyses permitting more detailed scrutiny of strategic subgroups. Table 4 indicates that changes in vote decisions among liberals, centrists and conservatives were differently influenced by the partisanship of the voters within each of these ideological groups. With the expected "normal" vote as the baseline for comparisons, it is clear that changes in vote choices between 1972 and 1976 were primarily a matter of most centrists and certain conservatives voting more Democratic and less Republican in the 1976 election. The largest changes were among centrist as well as conservative Democrats, centrist and conservative Independents, and among centrist Republicans. Liberals, including liberal Democrats, and conservative Republicans showed almost no net change in their vote decisions. Thus, while it is not incorrect to conclude that the pro-Democratic vote changes were to be found primarily among conservatives and centrists, such general conclusions obscure the important fact that there was almost no change among liberal Democrats or among conservative Republicans, who together make up some 40 percent of more of the voting public.

The more differentiated view of the electorate presented in Table 4 also reveals the extent to which those who switched their vote were concentrated among Democrats and Independents who constitute the continuing potential base for a national realignment and therefore for change in the partisan balance within the electorate. While it is true that we have documented change over only two elections, the relatively high proportion of conservative and centrist Democrats and conservative Independents who changed their votes in the short term to give greater support to the Democratic candidate raises questions about the likelihood of long-term changes that would result in party realignment. In 1972 it was the conservative Democrats who deviated most from their normal vote division, followed by conservative Independents and centrist Democrats. Conser-

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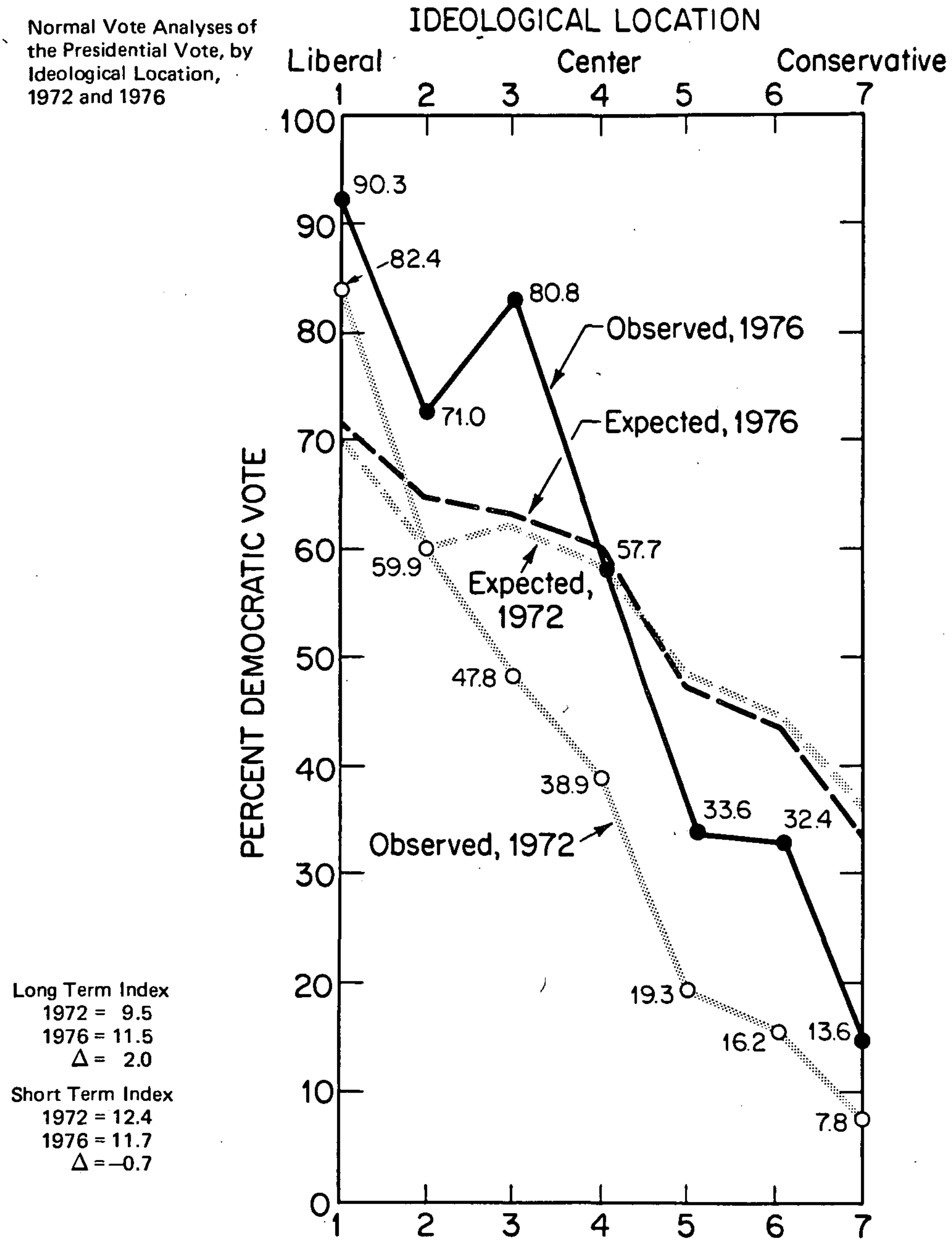
[15] The differences in language remarking on the "sharp increase" of Identifying Conservatives among Republicans (p. 12, above) and noting a "slight increase" in the present context is a result of the difference between emphasizing the diagnostically useful location of individual-level change and observing the net consequences of such change.

[16] The Boyd statistics for assessing the party-relatedness of ideological location and the party-independent contribution of ideological location to the vote choice indicates that the long-term index increased from 9.5 in 1972 to 11.5 in 1976 while the short-term index dropped only from 12.4 to 11.7. This article provides a different perspective on ideological voting from the investigation of the continuing impact of New Politics sentiments on the presidential vote we described in *Leadership and Change* (1977). Although New Politics sentiments exhibited a relatively constant relationship to liberal/conservative self-placement on the seven-point scale in the 1972–76 period, the independent contribution of the New Politics sentiments to the vote choice dropped sharply between 1972 and 1976. Since the two measures were intercorrelated at .33 in 1972 and .42 in 1976, the differences in their independent relationship to the vote in the two years suggests a clear difference in the extent to which the two electoral contests stimulates responses to two somewhat different dimensions of ideological thought.



	1	2	3	4	5	6	7
1972 $O_D - E_D$	12.1	0.7	–13.2	–19.1	–28.1	–27.8	–26.8
1976 $O_D - E_D$	20.1	7.4	18.7	–1.4	–12.7	–10.8	–18.5
Change in Difference, 72 → 76	8.0	6.7	31.9	17.7	15.4	17.0	8.3

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

Figure 1. Normal Vote Analyses of the Presidential Vote, by Ideological Location, 1972 and 1976

Table 4. Change in Deviations of Observed Democratic Vote from Expected Democratic Vote, 1972–76*

Ideological Location	Party Identification		
	Democratic	Independent	Republican
Liberal	+ 4 (18)	0 (3)	+ 8 (4)
Centrist	+28 (19)	+16 (6)	+15 (12)
Conservative	+28 (12)	+24 (3)	+ 2 (23)

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*A positive entry means the observed Democratic proportion of the two party vote increased, relative to the Normal Vote expected proportion Democratic, between 1972 and 1976. The approximate proportions of the classifiable portions of the population located in each cell of the cross-classification are shown in parentheses. Approximately 35 percent of the total electorate cannot be categorized because they report no ideological location and no party identification.

vative Democrats gave Nixon a bonus of 45 percentage points beyond their normal Republican vote. In 1976 they joined other conservatives, again favoring the Republican candidate, but this time with a bonus of only 17 percentage points over normal (a 28-point increase in their Democratic vote). Change between 1972 and 1976 in short-term deviations was in the direction of conservative Democrats "returning" to their partisan homes, even though that return fell 17 percentage points short of a full restoration to a normal party vote. Table 3 documents the familiar message that an electorate that is 3 to 2 Democratic and 2 to 1 conservative must have a lot of conservative Democrats; the normal vote analyses underlying Table 4 reveal both the propensity of these conservative Democrats to vote Republican and the volatility with which they undergo short-term change. The question of whether conservative Democrats will ultimately be moved to partisan realignment is central to the possibility of a major party realignment.

Ideological Location, Perceptions of Parties and Candidates, and Party Realignment

Much of the literature on the potential for realignment assumes that individuals experience tension when their ideological sentiments are incongruent or in conflict with their partisan positions. Since this tension is presumed to be unpleasant and uncomfortable, it is further presumed that individuals with dissonant political beliefs will reduce this tension by changing their partisan loyalties so that they become more consistent with their ideological views. This line of argument also assumes that there is wide consensus across the electorate as to which sets of views are consonant and which are dissonant. Many of the expectations of realignment have assumed that conservative Democrats must experience dissonance because the congruent patterns of political beliefs and values are found in the liberal-Democratic and conservative-Republican pairings.

However, one may reasonably suggest that the perceptions that create feelings of dissonance and congruence are, themselves, heavily influenced by voters' ideological and partisan positions. If so, many conservative Democrats may remain both conservative and Democratic because they do not perceive or experience any conflict between their partisanship and their ideological stance. They may perceive issues, parties and candidates in a manner consistent with both their partisan and ideological commitments, even though those perceptions may differ from the perceptions of most others.

Ideological views which deviate substantially from the "normal" range of views are often taken as *prima facie* evidence of ignorance about, lack of interest in, or a misunderstanding of partisan ideological reality. But these same "misperceptions," which are sometimes used as evidence of the absence of ideology, may also be seen as evidence of elaborate, well-integrated systems of political beliefs. Seemingly incongruent ties to an ideological location and a partisan commitment may lead to views of the political world which—even though "wrong"—are experienced as consistent by those holding them and indicate great involvement rather than lack of interest in politics.

For this argument to hold, ideological location must function, at least in part, as a filter through which parties and candidates are seen and evaluated. This argument also presumes that ideological location is a relatively fixed vantage point from which people view the political world. However, partisanship may be the more fundamental anchoring point. If so, perceptions of the partisan nature of candidates and issues would define the choice of ideological location, and the nature of the perception could not be attributed to the influence of a prior ideological location. In such a case ideological location would change with changing political circumstances and particularly with changes in perceptions of partisan-based politics. It is not our intention to try to identify those for whom partisanship is a more fundamental political anchor than ideological commitment, or vice versa. Moreover, we recognize that the relationships are complex, that partisanship and ideological beliefs function in different ways for different people, and that establishing causal links is most problematic. However, the data available to us do suggest that ideological location is a *relatively* stable and *relatively* independent vantage point from which many citizens view politics. The circumstances of the two elections of 1972 and 1976, in which the roles of candidates, issues and party loyalties were so different, provided one test of the stability of ideological location: the evidence suggests that location was not highly responsive to changes in the political situation. Thus, ideology may serve as a filter through which those citizens who hold "objectively" incongruent political views may organize the world of politics in ways that are consistent and congruent for them. And it may also serve as a basis for organizing the world for those citizens with "objectively" congruent political views as well. Table 5 demonstrates the extent to which ideological beliefs do, in fact, serve to orient voters to parties and candidates. It presents the joint relationship of one's own ideological location and partisanship to one's perceived proximity to each of the parties and to each of the candidates in 1972 and again in 1976.

As can be seen from Table 5, there is a strong relationship between where one places oneself ideologically and one's perceptions of ideological locations of the parties and their presidential candidates. In both years Democrats were consistently more likely than Independents or Republicans to see the Democratic party, rather than the Republican party, as close to their own ideological locations. In like manner, Democrats were more likely to see the Democratic candidate, rather than the Republican candidate, as close to their own ideological locations. Independents, particularly centrists

Table 5. Perceptions of Relative Proximities to Parties and Candidates vis-à-vis Ideological Location, 1972 and 1976*

	Party Identification					
	Perceived Proximity to Parties			Perceived Proximity to Candidates		
	Democratic	Independent	Republican	Democratic	Independent	Republican
1972 Ideological Location:						
Liberal	+77	+45	+52	+74	+43	+33
Centrist	+11	− 6	−26	−10	−21	−48
Conservative	−27	−64	−79	−50	−69	−86
1976 Ideological Location:						
Liberal	+84	+42	+36	+81	+31	+33
Centrist	+15	0	−14	+21	+10	−12
Conservative	−17	−44	−83	−14	−29	−77
Pro-Democratic Change:						
Liberal	+ 7	− 3	−16	+ 7	−12	0
Centrist	+ 4	+ 6	+12	+31	+22	+36
Conservative	+10	+20	+ 4	+36	+40	+ 9

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Entries for 1972 and 1976 indicate the preponderance of those who see the Democratic entity as closer (+) or the Republican entity as closer (−). The 1972–1976 change is scored so that an increase in the preponderance of those more proximate to the Democratic entity is (+) and an increase in proximity to the Republican entity is (−).

and liberals, were somewhat more likely than Republicans to see themselves as closer to Democratic rather than Republican positions, but clearly less likely to do so than Democrats. Republicans were most likely to see the Republican party and Republican candidates as close to their own ideological locations.[17]

The association between partisanship and perceived proximity to parties and candidates is overshadowed by the magnitude of the differences in perceived proximities across the ideological categories *within* partisan groups. Among Democrats, Independents and Republicans alike, those with liberal sentiments, when compared with centrists or conservatives, more often saw Democrats rather than Republicans as closer to their own ideological locations, while conservatives consistently located Republicans rather than Democrats as closer to their own ideological location.

The magnitudes of these differences indicate a linkage between the voters' own ideological location and their perceptions of the ideological location of the parties and their candidates. This linkage is stronger than the partisanship of the conservative Democrats and of the liberal Republicans in influencing their evaluations of parties and candidates. At the same time, as we have noted, their partisanship does modify these perceptions. In short, both party loyalty and sense of ideological location appear to influence perceptions of the relative ideological locations of both parties and candidates.

Perceptions of the relative ideological proximities of the two pairs of presidential candidates changed considerably between 1972 and 1976. The changes were concentrated among centrists of all partisan stripes and among conservatives, except for conservative Republicans. Table 5 demonstrates that assessments of McGovern and Nixon were different from those of Carter and Ford, and that the differences in assessments varied with the voters' own ideological and partisan positions. Furthermore, the magnitude of the differences indicates that differences between 1972 and 1976 were more pronounced because of the change in candidates than because of changes originating with parties. (The average change in the measures of proximity to parties was 9; in the measures of proximity to candidates, it was 21.) This is not surprising. Other studies have demonstrated both the continuity of evaluations of party and the variability introduced by the unique characteristics of each party's nominee. However, Table 5 provides important inferential evidence that the ideological assessments of the candidates, not the ideological assessment of the parties, produced the patterns of vote changes presented in Table 4.

This conclusion can be inferred from the fact that the changes in perception associated with the candidates rather than those associated with the parties (see the bottom three rows of Table 5) produce a pattern very similar to the pattern of entries in Table 4, entries which described changes in the deviations of observed vote from expected normal vote within the same partisanship-ideological location matrix. This similarity suggests that vote changes associated with the ideological location of voters were a function of voters' assessments of the ideological positions of the candidates. A more detailed exploration of how the candidates were perceived by citizens with different ideological sentiments further clarifies how a voter's own ideological position becomes a factor in evaluating the candidates' ideological sentiments. A new measure combining two questions in the 1976 CPS study that linked candidates to ideological positions was created for this purpose.

Perceptions of Candidates' Ideological Positions and Realignment

First, all persons who were able to place themselves on the seven-point scale bounded by "people whose political views" were either liberal or conservative, were asked to locate Ford and Carter, successively, on the same continuum.[18] Of the 54 percent of the total population who placed the two candidates at different locations on the scale, 46 percent located Carter as more liberal than Ford, while the remaining 8 percent saw Carter as more conservative than Ford.

Second, at a later point in the study, respondents were also asked to select from a list of groups (e.g., women, young people, farmers, southerners) those whom they expected that Carter, as president, would be likely to help. The same list was then presented again, and respondents were asked to select those groups who would be likely to be helped by Ford if he were elected. Liberals and conservatives were included in the list. Some 36 percent of the respondents volunteered the belief that Carter would help liberals or that Ford would help conservatives, or both; one in 20 (5 percent) thought that Carter would help conservatives or that Ford would help liberals, or both.[19]

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[17] Independent Leaners are included with other partisan groups; therefore the Independents consist only of "pure" Independent-Independents.

[18] Approximately 31 percent of the total population did not locate themselves on the initial seven-point item; an additional 8 percent of the total population could not locate either candidate; and approximately 7 percent placed both candidates at the same place on the seven-point scale. The proximity measures reported in Table 5 were derived from the locational data provided by the 61 percent of the total population who could locate the candidates as well as themselves.

We combined responses to perceptions of the candidates' liberal-conservative sentiments with judgments of which ideological group might be helped by which candidate to create a two-item summary measure of how voters saw the candidates aligned on liberal-conservative differences. This summary measure indicates that 29 percent not only saw Carter as more liberal than Ford but also thought Carter would help liberals or Ford would help conservatives, or both. Another 16 percent produced the same pairing, matching Carter with liberal views or Ford with conservative views, but did so on only one of the two measures, not on both. Eight percent of the total population had the order reversed on one or both of the measures; thus, for example, Carter was seen either as more conservative than Ford or as more apt to help conservatives. Five percent saw no difference on either measure, and 3 percent produced one ordering on one measure but the reverse on the other; that is, for example, they viewed Ford as more liberal and as more apt to help conservatives than Carter. The remaining 39 percent could not be categorized because they did not locate themselves or the candidates on the initial seven-point liberal/conservative scale.[20]

Table 6 presents the distribution by party identification and ideological location of that 45 percent of the electorate who not only saw Carter as more liberal than Ford but also thought Carter would help liberals or Ford would help conservatives, or both. It can be seen from Table 6 that perceptions of the candidates' ideological sentiments vary as a function of the perceiver's own partisanship and ideological location. Virtually all of the most liberal Democrats and a great many among the most conservative Republicans gave the same answers to both questions. They were most apt to locate the candidates, and they consistently paired Ford with conservative positions and Carter with liberal positions.

In this instance, as in all others in which respondents were asked to (a) locate the parties on the same dimension, (b) specify the relative policy preferences of the candidates, and (c) identify the relative issue positions of the parties, nearly all liberal Democrats and conservative Republicans had the same perceptions of

Table 6. Prevalence of Perceptions that Carter is More Liberal than Ford, 1976*

Ideological Location	Party Identification D_S	D_W	I_d	I_i	I_r	R_W	R_S
Ideological Liberal	88	83	91	33	**	**	**
Liberal	47	55	57	22	**	**	**
Liberal Preference	43	42	53	50	**	**	**
Centrist	28	25	24	13	21	19	**
Conservative Preference	31	34	18	19	22	46	39
Conservative	7	15	21	26	49	41	38
Ideological Conservative	**	36	40	47	87	57	74

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Entry is the proportion of the persons within the cell who rated Carter as more liberal than Ford on the seven-point liberal-conservative scale and who saw Carter as helping liberals or Ford as helping conservatives if elected.

**Too few cases to permit a reliable estimate.

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[19] Over half the population, 56 percent, saw no difference between the candidates, either seeing both as helping one or both groups or seeing neither as helping either liberals or conservatives. About 3 percent gave no response.

[20] Thirty-one percent did not classify themselves on the initial liberal/conservative self-placement question; 9 percent could not locate one or both candidates on that same ideological continuum.

both candidate and party positions, in 1976 as well as in 1972. Carter may have seemed fuzzy on the issues to many, and Ford's conservativism may have been blurred for some by the contrast with Reagan. But within the mass electorate these most ideological of voters, even though they held diametrically opposed ideological positions, agreed almost completely in their perceptions of the candidates' ideological sentiments. For conservative Republicans and especially for liberal Democrats, there was little ambiguity in their understanding of partisan differences whenever those differences were expressed in ideological terms.

However, this shared view of the world of partisan politics was not even the majority view in most other partisanship-ideology groupings. Although there are too few cases to evaluate the perceptions of liberal Republicans, relatively few of their ideological and partisan opposites—the conservative Democrats—shared the perceptions held by both liberal Democrats and conservative Republicans.

Table 7 also shows the divergence between the conservative Democrats and both the liberal Democrats and conservative Republicans in their perceptions of partisan ideological differences. This table is based on the 8 percent of the population that equated Democratic with conservative and Republican with liberal when locating the candidates. As Table 7 reveals, virtually no conservative Republicans or liberal Democrats held such views. Those heterodox perceptions were heavily concentrated in the conservative-Democratic portion of the matrix, along with occasionally disproportionate representation among some centrists and some Independents. This general pattern was found in every instance in which the ideological associations of parties, candidates and issue positions could be assessed, both in 1972 and 1976. Liberal Democrats and conservative Republicans have very similar perceptions of ideology-partisan linkages or associations, while conservative Democrats perceive a quite different structure of relationships among partisan and ideological alternatives.

The concentration of heterodox orderings among conservative Democrats is apparently not the result of several possibly confounding factors such as region or education. It is not because conservative Democrats are disproportionately from the South; northern Democrats more often have heterodox perceptions than do southern Democrats and northern conservative Democrats have them more often than southern conservative Democrats, particularly among those Democrats categorized as Ideological Conservatives. Contrary to what one might expect after reading the work of Converse and Klingemann, these heterodox perceptions are not associated with relatively limited education, limited exposure to the mass media, limited interest or involvement in political affairs, or limited political participation. Often as not, it is the best educated, most involved and most active among conservative Democrats who are most inclined to hold heterodox perceptions of the ideological locations of the parties and the candidates. This apparent contrast with earlier work may not seem quite so startling once it is recognized that the present analysis is confined to those persons who could be classified with our new measure of ideological location. The 34 percent of the electorate who were not so classified (see Table 1) may well include many persons who, if asked to assign ideological labels to parties and candidates, would often

Table 7. Prevalence of Perceptions that Ford is More Liberal than Carter, 1976*

	Party Identification						
Ideological Location	D_S	D_W	I_d	I_i	I_r	R_W	R_S
Ideological Liberal	0	0	7	8	**	**	**
Liberal	9	7	8	16	**	**	**
Liberal Preference	19	11	19	5	**	**	**
Centrist	8	14	17	8	24	13	**
Conservative Preference	15	16	0	19	9	6	5
Conservative	38	19	17	4	5	5	12
Ideological Conservative	**	24	24	14	0	6	2

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Entry is the proportion of persons within the cell who rated Ford as more liberal than Carter on the seven-point liberal-conservative scale or who saw Ford as helping liberals or Carter as helping conservatives if elected.

**Too few cases in cell to permit a reliable estimate.

produce heterodox orderings out of confusion or ignorance. Such persons are also limited in their political resources and activities (see Table 2). It is likely that they have been included in earlier work that used "mislabeling" as a criterion of ideological comprehension and political sophistication. Thus, studies of citizens with heterodox views may have grouped together two very different types of citizens: the genuinely apathetic, or ignorant for whom ideology is irrelevant, and the active and informed for whom ideology provides a frame of reference from which seemingly heterodox views are perceived as reasonable, consistent and appropriate. Future studies of ideological beliefs must distinguish between these two types of citizens.

The same caveat may also need to be extended to other categories of response. Among liberal Democrats the incidence of "No Difference" or "Don't Know" responses is a direct function of lack of education, little use of the media, minimal involvement or participation; the proportions of such responses increase as education, media use, involvement and participation decrease. However, among conservative Democrats, these relationships are weakened and, at times, even reversed. As with reversals in the ideological orderings of political objects, the absence of perceptions differentiating party and candidate ideological location may indicate something other than error born of ignorance or indifference. As with heterodox ordering, non-ordering may be a function of a clearer understanding of and interest in politics and a higher level of ideological sophistication than has usually been attributed to citizens making such responses.

None of this denies the conclusion that deviations from the orthodox, from perceptions such as those shared by liberal Democrats and conservative Republicans, are often—and undoubtedly most often—indications of limited ideological understanding. For example, among those who are classified as centrists because they do not label themselves as ideologues, ideology is, as would be expected, of limited relevance in evaluating the rest of the political world. Among the centrists, the "don't know" response is common, as are inconsistent portrayals of party and candidate positions. Their various patterns of ordering parties' and candidates' ideological locations probably reflect confusion, inattention, indifference, and a limited grasp of the ideological elements of politics.

Taken together, Tables 6 and 7 suggest that conservatives and liberals perceive the world of politics with persistently ideological frames of reference. They are, in short, responding predictably both if one knows that they hold ideologically strong and consistent views and if one assumes that these views serve as a context within which to perceive and evaluate political issues, people, and ideas. However, those ideologues whose ideological positions seem to come into "conflict" with partisan commitments hold perceptions of the political world different from the perceptions shared by other ideologues. While many conservative Democrats and liberal Republicans report that they don't know the candidates' ideological locations, a substantial minority hold views about the ideological positions of the candidates that are opposite of those reported by other ideological groups in the electorate.

We do not know the extent to which conservative Democrats and liberal Republicans experience tension because of a divergence between their partisan loyalties and their ideological commitments. Nor do we know, if tension exists, how it is resolved—if at all. Although one may plausibly suggest that an ideological preference may serve as a filter or frame of reference through which political candidates, parties, issues, and events are perceived and evaluated and that this frame of reference may provide a consistent, coherent view of politics, no one has yet developed relevant and direct measures of the existence of tension and of the processes by which it may be reduced. We do know that conservative Democrats and liberal Republicans demonstrate a heterodox structure in their perceptions of political phenomena. However, as we noted earlier, much of the theorizing about the prospects for partisan realignment has presumed that conservative Democrats have the same view of the political world as do liberal Democrats and therefore feel some pressure to achieve consistency by changing their partisanship to match their ideology. The data we have examined suggest that many individuals thought by the political analyst to experience inconsistency, and therefore tension, have instead achieved some congruence between their partisanship and their ideological positions. The vast majority of conservative Democrats view parties, candidates, and issues in a way compatible with both their partisan and ideological commitments.

In sum, the continued absence of the partisan realignment which would result from conservative Democrats—or liberal Republicans—shifting their party loyalties may be partially explained by their unique perceptions of the ideological locations of the parties and the presidential candidates. In 1976, liberal

Democrats and conservative Republicans were nearly identical in their perceptions of the ideological differences separating Carter from Ford and Democrats from Republicans. Yet, relatively few conservative Democrats joined this consensus. Instead, most conservative Democrats perceived the ideological locations of both the parties and their candidates so as to either eliminate or greatly reduce the possibility of conflict between their party loyalties and their ideological commitments. Therefore, it is reasonable to suggest that only the minority who share the dominant perceptions of the liberal Democrats and conservative Republicans (as shown in Table 6) are likely candidates for a change in partisanship that might produce some degree of party realignment.

These results suggest not only substantive conclusions about the potential for realignment but also methodological concerns about the meaning of some of the common measures of ideology. Those who hold heterodox political beliefs may do so because they are highly ideological or because they are uninformed and lack interest. If both a highly developed ideological filtering system and a confused or apathetic posture toward politics can be responsible for heterodox views of the positions of parties and candidates, analyses must separate the two. Further, correlational tests of constraint will be flawed if heterodox ideologues are not separated from orthodox ideologues. However, the heterodox ideologues would be hard to identify in most previous correlational tests of ideology precisely because the inversions indicating their ideological commitment are the same inversions expected among the nonideological, uninterested members of the electorate.

Ideological Location and Some Implications for Issue Voting

It is often simply presumed that ideological sentiments represent some summary position on numerous questions of government policy. We have suggested, however, that the concept of ideological location (like the concept of party identification) may have many meanings or functions, only one of which is as an abstract or general summary of discrete policy positions. Ideological position is correlated with many different expressions of policy preferences (see Miller and Levitin, 1977, p. 264). As Table 8 shows, there was a modest relationship, both in 1972 and 1976, between ideological location and several statements of policy preferences. The correlations with issue positions are also reasonably stable across the same period. At the same time, the correlations are of a relatively modest magnitude; they are certainly too small to warrant the conclusion that ideological location is simply a summary of policy preferences (but cp. Miller et al., 1976, pp. 776–78).

The certain error of that conclusion becomes apparent as we look next at how individuals link their own policy preferences to the ideological meanings they give to policy alternatives. In both 1972 and 1976 respondents were asked what positions liberals and conservatives would take on each of the two different issues. Table 9 presents proximity measures which join the voters' issue positions and the voters' understanding of the positions of liberals and conservatives on these issues.

When Table 9 is compared with Table 5, it is clear that differences between the relative proximities to perceived liberal and conservative positions on issues (within party iden-

Table 8. Relationship Between Ideological Location and Policy Preference on Nine Issues, 1972–1976*

	Jobs and Standard of Living	Rights of the Accused	Busing	Aid to Minorities	Governmental Health Insurance	Urban Unrest	Tax Policy	Equal Role for Women	Legalization of Marijuana	Average
1972	.31	.32	.38	.37	.28	.29	.12	.20	.35	.29
1976	.32	.22	.26	.31	.34	.30	.16	.21	.35	.27

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*Entry is the correlation between ideological location on the seven-point continuum and policy preferences on seven-point issue scales. "No Opinion" and "Don't Know" were treated as missing data. The issues in this table are all of the policy questions presented both in 1972 and in 1976 in the seven-point scale format with a clear policy alternative specified at each end of the scale.

tification) are much smaller than the comparable differences between the relative proximities to the parties' and candidates' ideological positions. That is, liberals and conservatives are less apt to differentiate the particular issue positions of other liberals and conservatives (and thereby establish their own proximity to the liberal, or conservative, alternatives on issues) than they are to differentiate the ideological locations of parties and candidates. It would appear that the parties and the candidates are more likely to be drawn into structures of perceived ideological relationships and linkages than are ideologically defined policy alternatives. If policy questions are less associated with ideological sentiments than are parties and candidates, as these data suggest, then policy positions alone may be of relatively limited importance in developing and maintaining an ideological stance. This conclusion is, of course, contrary to conclusions drawn from both empirical studies and conventional wisdom about the basis and content of mass political ideology where there is the pervasive assumption that ideology is based entirely on issue preferences.[21]

The second conclusion to be drawn from Table 9 is that party identification may have a direct influence on perceptions of the ideological meaning of policy alternatives. The proximity measures in Table 9 link respondents' own issue positions to the positions they attribute to liberals and conservatives. But these proximity scores vary directly with party identification. Specifically, Republican liberals are more likely to see conservatives as closer to their own issue positions than are Democratic liberals, and Republican conservatives are more likely than Democratic conservatives to see conservatives as closer to their own issue positions. It may be that one of the meanings of an ideological position—independent of what it may mean for policy preferences—is what it may mean for partisanship. Ideology and partisanship are tightly linked. It is as though party identification denotes responses to political parties and connotes ideological responses as well. This ideological aspect of party loyalty is so strong that, for example, Democrats (even if

Table 9. Perceptions of Relative Proximity to Liberals and Conservatives on Four Selected Issues, within the Joint Distribution of Party Identification and Ideological Location*

	Party Identification		
Ideological Location	**Democratic**	**Independent**	**Republican**
Jobs and Standard of Living, 1976			
Liberal	+35	+ 5	– 6
Centrist	– 2	– 6	–17
Conservative	–27	–24	–58
Rights of the Accused, 1976			
Liberal	+35	+18	– 2
Centrist	–13	+ 1	– 9
Conservative	–23	–31	–39
Campus Unrest, 1972			
Liberal	+41	+16	–14
Centrist	–17	–27	–41
Conservative	–47	–45	–66
Inflation, 1972			
Liberal	+38	+20	+14
Centrist	+ 4	– 4	– 3
Conservative	–22	– 8	–27

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

*A (+) entry is the preponderance of those who see themselves as closer to liberals than to conservatives; (–) indicates a preponderance of those who see themselves as closer to the conservative position.

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[21] It may be that these surprising results reflect a selection of policy questions with little ideological meaning. However, this does not seem likely, given the correlations of ideological location with issue position described in Table 8. It seems more appropriate to suggest that the substantive meanings, the connotations, and the implications of ideological locations are diverse and include far more than a summary measure of policy preferences.

they are conservative Democrats) more often than Republicans describe their issue preferences as closer to the issue positions of liberals than of conservatives. And if liberals with an affinity for other liberals are Republicans rather than Democrats, they are apt to describe their issue positions as relatively closer to conservative than to liberal positions. In sum, when people describe themselves as having an ideological position, they also seem to be saying something about their positions on the parties (and presidential candidates) quite apart from their issue or policy stands. This provides further evidence for questioning the conclusion that an ideological position is entirely, or perhaps even primarily, a summary measure of positions on public policy.[22]

Table 10, which indicates a need for a more complex and inclusive interpretation of ideological location, provides the most direct test of the extent to which policy positions are—or are not—seen as measuring or implementing ideological sentiments. This table reveals the extent to which an ideological stance is associated with describing federal socioeconomic policy alternatives in ideological terms. If issue voting is a direct translation of ideological concern, then the most ideological voter should be able to link policy positions and ideological positions clearly and coherently. Moreover, both liberals and conservatives ought to agree as to which of the policy alternatives on a particular issue is liberal and which is conservative. Table 10 is based on only those citizens with some college education who, by virtue of that education, are presumably most skilled in linking abstract ideology to a specific policy question. The table shows that many of those citizens were unable to assign ideological labels to policy alternatives or make correct assignments, even though the question of the role of the government in guaranteeing jobs and a good standard of living has been an enduring source of partisan ideological dispute over the years.[23]

There was, however, a high degree of agreement about the policy-ideology linkage among

Table 10. The Perceived Association of Liberals and Conservatives with Positions on the Question of Governmental Guarantees of Jobs and a High Standard of Living, among Persons with at Least Some College Education

	Liberal is Pro Governmental Guarantee; Conservative is Pro Self-Help	Liberal is Pro Self-Help; Conservative is Pro Government	No Difference Between Liberal and Conservative	Don't Know Liberal or Conservative Position	Total
Ideological Liberal	78%	11	2	9	100%
Liberal	50%	22	12	16	100%
Liberal Preference	51%	16	11	22	100%
Centrist	30%	19	15	36	100%
Conservative Preference	50%	15	16	19	100%
Conservative	56%	13	6	25	100%
Ideological Conservative	81%	4	1	14	100%

Source: Data are from the 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

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[22] The evidence based on the contrast between intra-column differences of Table 9 and Table 5 may be less conclusive than that provided by the intra-row differences among partisans in Table 9 alone. This is true because, as we will note shortly, even the most ideological are apparently often unclear about what constitutes a liberal position (or a conservative position) on an issue, and it is possible, although we have not explored the possibility, that both liberals and conservatives, holding appropriate policy preferences themselves, misperceive positions of other liberals and conservatives often enough to reduce the frequency with which they see their own group as the more proximal. This would limit the intra-column differences on issues without necessarily demonstrating anything about the relative prominence of parties, candidates and issues in one's ideologically structured belief system.

[23] The question was: "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose that these people are at one end of this scale—at point number 1. Others think the government should just let each person get ahead on his [sic] own. Suppose that these people are at the other end—at point number 7. And, of course, some other people have opinions somewhere in between. . . ."

the most ideological of these citizens. About four out of five of the most conservative and of the most liberal agreed that the liberal position was that of "supporting a governmental guarantee of full employment and a high standard of living," while the conservative position favored "allowing each person to get ahead on his [sic] own." The less extreme ideologues agreed far less often among themselves about the ideological labels for policy alternatives. The number of respondents who actually reversed the ideological positions, or said they did not know the liberal or conservative position, is particularly interesting; about a fifth of the most ideological conservatives and liberals gave one of these responses. Partisan disagreements, often couched in ideological terms, have, over the last four decades, centered on the government's role in guaranteeing full employment and insuring a high standard of living. Yet, in 1976, the highest levels of ideological commitment were required before substantial proportions of citizens with some college education were able to specify the liberal-conservative alternatives on this question of economic and social policy. And, even among the most ideological, about a fifth did not make the correct linkage.

Three other policy questions are treated more summarily in Table 11. The 1972 perspectives on the proper treatment of unrest on the nation's college campuses were those most clearly seen in ideological terms. And it was the college-educated among the liberals and conservatives who were most apt to make the correct link between policy alternatives and ideological position. Even the policy alternatives on the question of the rights of the accused, an issue central to much of the ideological debate over the law and order in the period from the middle sixties to early seventies, were in 1976 only moderately well understood in ideological terms. Again, it is the college-educated who were most apt to associate the policy alternatives posed by this issue with the correct ideological position.

Elections as Policy Mandates

Ideological location is an important factor in shaping voters' choices on election day. In both 1972 and 1976 ideological location made a significant contribution to the vote outcome. And that contribution was independent of party identification. In both years ideological location was also associated with differences in perceptions of the ideological positions of the parties and their candidates. But it does not necessarily follow that clear policy mandates can be inferred from such "ideologically" based voting. To the contrary, we have shown that there are no simple relationships among ideological location, perceptions of the ideological meaning of issues and the substance of specific policy preferences.

It is true that a substantial minority of that electorate, perhaps 15–20 percent, hold views suggesting that their policy preferences and their perceptions of party and candidate issue positions are based on coherent ideology. At the other extreme, an even larger minority, at least 30–35 percent of the population, show no interest in organizing their preferences and perceptions of the political world in ideological terms. The balance, 45–55 percent, seem to have political perspectives which, however they may serve those individuals, do not permit aggregation into more general, ideological perspectives with common meanings. These individuals describe themselves with ideological labels, often hold attitudes similar to those of "real" ideologues, and may vote similarly; but unlike "real" ideologues they do not have coherent, well-articulated views of the political world. Their presence in the electorate suggests that even an electoral victory heavily influenced by the ideological self-labeling of the voters may defy interpretation in terms of a policy mandate. Conversely, issue voting may be most difficult to summarize with an ideological terminology of uniform relevance even to those who share policy preferences.

Apparently, the translation of policy preferences into ideologically based partisan electoral choices is subject to considerable slippage, primarily because most voters are unable to distinguish the liberal from the conservative positions on many issues. Apparently only the best-educated among the most ideological of voters understand the ideological implications of their policy preferences clearly enough to link themselves ideologically to policy preferences and to party or candidate as well as, ultimately, to the vote decision. Thus, one frequent break in this chain occurs when voters attempt to associate policy alternatives with liberal or conservative issue positions. The meaning of the strong relationship between ideological location and the vote is, therefore, open to many interpretations; however, it is clear that issue voting and ideological voting are not coterminous, and that an ideological position is not simply a summary statement of many issue positions. Our demonstration of the extent to which ideological sentiments do not translate directly into policy preferences is entirely congruent with the research of Converse, Klingemann, Stimson and others who

Table 11. The Perceived Association of Liberals and Conservatives with Particular Positions on Issues, within Educational Levels

	Campus Unrest, 1972[a]			Rights of the Accused, 1976[b]		
Ideological Location	College	High School	Grade School	College	High School	Grade School
Ideological Liberal	96	89	–[e]	78	68	–
Liberal	89	49	41	53	37	28
Liberal Preference	88	44	–	51	49	–
Centrist	64	41	35	40	24	13
Conservative Preference	83	52	–	42	32	–
Conservative	78	67	47	62	35	27
Ideological Conservative	88	79	43	70	61	37

	Jobs and Standard of Living, 1976[c]			Inflation, 1972[d]		
Ideological Location	College	High School	Grade School	College	High School	Grade School
Ideological Liberal	78	22	–	63	58	–
Liberal	50	35	30	55	36	28
Liberal Preference	51	47	–	31	36	–
Centrist	30	28	26	30	11	5
Conservative Preference	50	30	–	26	24	–
Conservative	56	42	23	22	22	18
Ideological Conservative	81	67	54	35	12	9

Source: The data are from the 1972 and 1976 National Election Studies carried out by the Center for Political Studies of the University of Michigan, Institute for Social Research.

[a] Entry is proportion who associate liberalism with support for protesters, maintaining law and order with conservatism.

[b] Entry is proportion who associate liberalism with concern for the civil liberties of the accused, reduction of crime with conservatism.

[c] Entry is proportion who associate liberalism with active role for government, individual self-help with conservatism.

[d] Entry is proportion who associate liberalism with government action against inflation, no immediate government action with conservatism.

[e] Too few cases for reliable estimation of entry.

have documented the lack of ideological thinking in mass electorates in the United States and Europe. The explanation of the various linkages and breaks in the linkages among policy preferences, vote choices, perceptions of candidates and partisanship for voters with various ideological sentiments provides a research agenda for the future.

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Gender Roles and Party Roles*

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This article uses discriminant analysis to assess sex and party differences across four aspects of party organization: incentives for participation, party role definitions, party activities and electoral ambition. The results illustrate that gender roles operate most distinctly in the two areas of electoral ambition and party activities. Party context is more important than sex in terms of the other two areas, party role definitions and incentives for participation. The authors conclude that party activists' gender-related behavior can be better understood by discovering how the context of party organization either modifies or reinforces such behavior.

Though an increasing number of women are active in party politics, their emergence into the ranks of the politically active does not appear to be totally unrestricted by traditional gender role expectations.[1] Even where there is a conscious attempt to promote equality between the sexes, women still tend to perform routine tasks while men tend to dominate the functions surrounding organizational goals and political candidacy (e.g., Maisel, 1975, pp. 202–08). This research examines gender roles and party roles in one southern party setting. The analysis differs from previous work in two respects. First, the usual bivariate correlations of sex with other variables are replaced by multivariate analysis in an attempt to map the overall pattern of hypothesized gender role differences. Second, the study moves away from a purely sociological perspective to a consideration of the effects of Republican or Democratic organizational context and asks, in effect, "What are the motivations, roles, activities and career aspirations that differentiate, respectively, Democratic and Republican men and women activists?"

The research perspective draws upon explanations of gender differences offered in previous studies of party activists (Jennings and Thomas, 1968; Costantini and Craik, 1972). Employing the Parsons and Bales theory of *expressive* and *instrumental* functions, Costantini and Craik (1972, p. 235) posit a parallel between the family and party organization: "The male party leader, like the husband, is more likely to specialize in the instrumental functions . . . those functions related to the external world. The female party leader, like the wife, tends to specialize in expressive functions . . . those concerned with . . . internal affairs." To the extent that party activists receive and follow the gender role socialization traditionally prescribed for each sex, women would be expected to be more expressive and men more instrumental across four aspects of party organization: the pursuit of electoral careers, incentives for participation, party role definitions and performance of party duties.

The instrumental nature of political ambition is quite straightforward. As in the family, presumably some party members will spend a life of support within the organization (expressive roles) while others will venture forth in pursuit of career goals outside the basic unit (instrumental roles). The organizational "family" promotes the extra-organizational political careers of its ambitious members, who are expected to be men.

In a similar vein, the standard three-part division of incentives for participation (Clark and Wilson, 1961) can be divided into *instrumental/expressive* categories. Material motives

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[1] Existing research focusing directly on women in party organization is limited. The work most relevant to this undertaking includes Jennings and Thomas (1968), Costantini and Craik (1972) and Lee (1976). A larger body of literature describing the incidence of women in party and elective leadership roles and as convention delegates includes the work of Johnson and Stanwick (1976), Huckshorn (1976, pp. 25–26), Lynn and Flora (1977), Kirkpatrick (1974), Kirkpatrick (1976), Gruberg (1968) and Jaquette (1974).

involving the pursuit of individual monetary and career rewards would be instrumental, while solidary or social considerations would be expressive. Purposive incentives involving attempts to affect public policy by using the organization to alter the direction of government could be considered instrumental.

Role definitions and activities in party studies typically fall into an *organizational/electoral* dichotomy.[2] This dichotomy closely parallels the expressive/instrumental breakdown, with electoral activities in effect representing instrumental efforts. A more fundamental dimension, however, may cut across organizational and electoral categories. Both organizing and electoral work involve perfunctory tasks as well as nonroutine activities. The rather routine task of attending meetings is to be contrasted with the more innovative work involved in recruiting new members and expanding the organization. Electorally, tasks such as putting out the mail must be contrasted with recruiting candidates and raising money. In this study, *routine* organizational or electoral roles and activities are considered expressive, while *nonroutine* organizational or electoral roles and activities are considered instrumental.

Because this study is concerned with building a political taxonomy of the sexes, we must also consider a second category, party membership. Much evidence illustrates the differences between the parties in recruiting and other activities, which depend upon majority-minority status, the social setting and the like (Crotty, 1967; Pomper, 1965; Patterson, 1963; Bowman and Boynton, 1966; Conway and Feigert, 1974; Ippolito, 1969; Althoff and Patterson, 1966; Beck, 1974). Although some of this evidence is contradictory and some difficult to compare, the important point from our perspective is that gender roles do not operate in a political vacuum. It is to be expected, for example, that one party's emphasis on organizational as opposed to electoral goals or varying emphasis between the parties on the cultivation of political careers could create more party than gender differences.

Methodology

The Data. The data for this study come from a survey of Democratic and Republican political party officials in Fulton and DeKalb Counties, Georgia, conducted from January to late April of 1977. The counties contain approximately two-thirds of metropolitan Atlanta's 1.5 million population. The Atlanta area supports viable Republican organizations that have emerged as successful competitors with the Democratic organizations, especially at the local level, although the Democrats remain the dominant party.

The study focused on the people at the base of the four organizations because of the comparability of positions across parties and because the greatest proportions of women are found at the lowest levels. While there are some differences in the organizational groupings of workers, all four organizations attempt to secure precinct leaders. Many precinct leaders also occupy higher positions, especially in the Democratic party, but most are active only at the lowest level. Using official party lists, we sent letters to all precinct people. Trained interviewers then attempted to contact and interview all potential respondents. The standard survey schedule took an average of one hour to complete. Of 419 potential respondents, 286 were reached and interviewed—a success rate of 68 percent.[3] Of this group 27 respondents provided incomplete information and were consequently eliminated from the data set. The respondents analyzed here include 78 Democratic men, 56 Democratic women, 66 Republican men and 59 Republican women. The questionnaire included items appropriate to the four major areas of concern. Respondents were provided with a series of reasons for their continuing involvement in party politics and were asked to rate the importance of each.[4] We asked open-ended questions to tap role perceptions and administered a series of specific items on actual activities performed.[5] As for political ambition, each respondent was asked if s/he were interested in running for office if the opportunity arose.

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[2] Other roles and activity dimensions can be isolated, of course. Ideological or opinion leadership, for example, is oftentimes separated from other activities, but such activity does not constitute a large proportion of the party worker's time, particularly at the precinct level (Conway and Feigert, 1974; Eldersveld, 1964).

[3] Thirteen percent of the party officials refused to allow an interview. The remaining 19 percent could not be reached and/or interviewed because of change of address, inaccuracies in party lists or illness. However, those completing the interviews closely resemble the known characteristics of the larger population of Atlanta party activists, although blacks, men, Republicans and Fulton County officials are slightly under-represented.

[4] Respondents were asked to rate the importance of the following on a one to five (low to high) scale: job experience, opportunity to run for elective office, to affect government policy, and to make business and professional contacts, loyalty to the party, the excite-

Data Analysis. We used discriminant function analysis to distinguish local party activists on the basis of their sex and party.[6] The maximum number of functions that can be derived is equal to one less than the number of groups. In the present study, discriminant function analysis can distinguish the four groups—Democratic men, Democratic women, Republican men, Republican women—by up to three linear functions best able to capitalize on the differences among the groups. Since the formal logic of the discriminant function model is not disturbed by treating independent criterion variables as "dependent" and thus to be "predicted" by what normally would be considered dependent variables (Cooley and Lohnes, 1971), the method affords an opportunity to assess the magnitude of the differences between the characteristics of the criterion groups. Thus, the investigation seeks to determine patterns of group differences by treating two dichotomous variables, sex and party affiliation, as "dependent" and employing the party organization variables described earlier to separate the group of political party activists into their correct sex and party categories.[7] Since membership in the four groups is known a priori, the ability of the discriminant function(s) to categorize Democratic and Republican men and women will serve to evaluate the impact of gender role and party on political behavior in party organizations.

Findings

The findings support the expectation that gender roles are important in the life of the party organization but that the form of gender role relationships may be modified by the party context. The discriminant function analysis produces two functions. The first function is dominated by the enjoyment of politics as an incentive, but also contributing are the incentives of the opportunity to make business contacts and the chance to run for office, and the routine-nonroutine nature of organizational role definitions (see Table 1).[8] The second function is dominated by electoral ambition, but also contributing are the activities of attending party meetings, telephoning and canvassing, and the routine-nonroutine nature of electoral role definitions. The incentive of desiring to make business contacts also appears on the second function (see Table 2). The group means in Table 1 show that the mean

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ment of politics, the enjoyment of working with others. The coding scheme for this article was ordered to be consistent with the expressive/instrumental dimension, with the latter scored high. The last three incentives—loyalty, excitement and enjoyment—were reverse-coded to provide consistency across all items.

[5] Up to four responses were coded on the role perceptions. The responses were classified as organizational or electoral, summed and given value consistent with the expressive/instrumental dichotomy. The open-ended responses also were divided in terms of a routine/innovative breakdown such that routine organizational responses (e.g., attendance at meetings) were differentiated from innovative roles (e.g., recruitment of new members); routine electoral roles (e.g., telephoning for candidates) were distinguished from innovative electoral roles (e.g., candidate recruitment). In both routine/innovative measures, the former was subtracted from the latter. As for activities, several objective questions were employed. The instrumental activities asked about were: raising money, talking to public officials, number of recruitment conversations, having run for office in the past. The expressive activities were: incidence of telephoning experience, transporting of voters, attendance at party meetings, routine between-election work within the party. The expressive activities were reverse coded.

[6] The analytical potential of discriminant function analysis is threefold. It is a more appropriate technique than is multiple regression when the criterion variables in question are dichotomous (Kort, 1973) as party and sex in the present study. It can illustrate the relative contribution of the discriminating variables (Cooley and Lohnes, 1962, pp. 116–33). It may be used to classify groups, whether or not a priori probabilities of membership are known (Hoel, 1954, pp. 179–84; Anderson, 1958, pp. 133–52; Cooley and Lohnes, 1962). Discriminant analysis assumes a multivariate normal distribution and equal variance-covariance across all groups. However, Cooley and Lohnes (1971, p. 228) contend that the issue of homogeneity of group dispersions might be ignored as the technique is "fairly robust." The covariance matrices of the four groups in this study were nonetheless examined, and the degree of equality was judged to be within acceptable limits.

[7] Rao's *V* was employed as the criterion for including variables in the discriminant function (Klecka, 1975).

[8] In each function, some party variables make larger contributions than others to the function's discriminating ability, and the party variables are arranged according to the absolute magnitude of their standardized discriminant function coefficients. Those coefficients greater than $|.25|$ are the basis for the characterization of each function.

Table 1. Distinguishing Characteristics of the Parties: Incentives and Roles

Discriminating Party Variables	Standardized Discriminant Function Coefficients Function I Incentives and Roles
Incentive: Enjoying politics	.53956
Role: Defining role as routine-nonroutine organizational	.37611
Incentive: Desiring to make business contacts	–.37103
Incentive: Having the chance to run for office	–.27391
Ambition: Expressing aspirations for office	.24094
Activity: Telephoning	.23740
Incentive: Expressing party loyalty	.22014
Activity: Transporting voters	.21233
Role: Defining role as organizational	.21179
Role: Defining role as electoral	.20335
Activity: Registering voters	.19938
Activity: Talking to public officials	–.18364
Activity: Taking initiative in recruiting candidates	.18106
Incentive: Finding politics exciting	–.17252
Activity: Canvassing	–.10986
Role: Defining role as routine-nonroutine electoral	.10364
Activity: Collecting money	.08646
Activity: Attending party meetings	.00811

Group	Means
Democratic Women	–.879
Democratic Men	–.181
Republican Women	.244
Republican Men	.741

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

discriminant function scores for Democrats (both men and women) are negative and analogous scores for Republicans are positive, thus suggesting that the first function distinguishes between political parties. The group means in Table 2 show that the mean discriminant function scores for women (both Democratic and Republican) are negative and the scores for men of both parties are positive, thus suggesting that the second function distinguishes between the sexes.

The predictive power of the two discriminant functions taken together is demonstrated in Table 3. On the basis of random assignment to four groups, one should be able to predict correct group membership 25 percent of the time. The use of the discriminant functions raises predictive ability to 57 percent, on the average. As Table 3 shows, the membership group of Democratic men is most easily predicted, and the least easily predicted membership group is Republican men.

The relative importance of the two functions can be determined by their ability to explain variance in all the discriminating variables and by their relationship to the four groups being distinguished. The first function, distinguishing between the parties, with an eigenvalue[9] of .496, explains 52.6 percent of the variance existing in the discriminating variables. The relationship between this function and the four groups is represented by a canonical correlation of .576. The second function, distinguishing between the sexes, with an eigenvalue of .401, explains 42.5 percent of the variance. Its relationship to the four groups is represented by a canonical correlation of .535. Together these two functions explain about 95 percent of the variance in the discriminating party variables.[10]

The findings suggest that party activists can

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[9] The eigenvalue of a discriminant function represents the part of the variance of all the discriminating variables explained by that function.

[10] A third function, explaining 5 percent of the variance, was considered insignificant and is not presented or discussed.

Table 2. Distinguishing Characteristics of the Sexes: Ambition and Activities

Discriminating Party Variables	Standardized Discriminant Function Coefficients Function II Ambition and Activities
Ambition: Expressing aspirations for office	.55978
Activity: Attending party meetings	.36430
Activity: Telephoning	.35627
Incentive: Desiring to make business contacts	.29833
Activity: Canvassing	.27326
Role: Defining role as routine-nonroutine electoral	.27180
Role: Defining role as routine-nonroutine organizational	−.23715
Activity: Collecting money	.21908
Incentive: Finding politics exciting	.19887
Incentive: Enjoying politics	.15717
Role: Defining role as organizational	−.14668
Incentive: Expressing party loyalty	−.09247
Activity: Registering voters	.08414
Activity: Taking initiative in recruiting candidates	.07987
Role: Defining role as electoral	−.06569
Incentive: Having the chance to run for office	.04323
Activity: Talking to public officials	−.03305
Activity: Transporting voters	−.00301
Group	**Means**
Republican Women	−.778
Democratic Women	−.262
Republican Men	.152
Democratic Men	.648

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

be distinguished by party and by sex. But which party members exhibit the more instrumental or expressive incentives and role definitions? Which sex is more ambitious and more responsible for the instrumental or expressive activities? To answer these questions and to relate the findings to the theoretical perspective of instrumental and expressive gender and party roles, we must consider the instrumental/expressive nature of the scoring for party variables and their positive or negative contribution to each set of individual discriminant function scores. The instrumental/expressive scoring of the party variables is illustrated in Table 4, which displays the mean scores on each discriminating variable for men and women of the two parties. The scoring procedure is such that instrumental values are always scored "high" and expressive values "low." Thus, for example, those attending a high number of organizational meetings actually are scored lower than those attending few or no meetings.

The contribution of each party variable to each individual's discriminant function score is

Table 3. Proportions of Correct Classifications by Two Discriminant Functions

Actual Group	N	Percent Correctly Predicted (N)
All Respondents	259	57.1 (148)
Democratic Men	78	65.4 (51)
Democratic Women	56	51.8 (29)
Republican Men	66	45.5 (30)
Republican Women	59	55.9 (33)

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

Table 4. Discriminating Variables[a] with Mean Scores for Each Group

Party Organization Variables	Democrats Men	Democrats Women	Republicans Men	Republicans Women
Incentives				
Having chance to run for office	2.44	2.07	1.76	1.39
Desiring to make business contacts	2.67	2.63	1.94	1.49
Expressing party loyalty*	2.31	1.88	3.02	2.27
Finding politics exciting*	2.68	2.09	3.26	2.64
Enjoying working with others in politics*	2.22	1.43	2.92	2.12
Role Definitions				
Organizational*	2.44	2.27	2.86	2.63
Electoral	.87	.63	1.02	1.07
Routine-nonroutine organizational	.33	.32	.42	.61
Routine-nonroutine electoral	.29	.09	.23	.19
Activities				
Telephoning*	.23	.07	.30	.07
Canvassing	.78	.73	.60	.58
Transporting voters*	.63	.64	.85	.69
Collecting money	.56	.46	.55	.53
Registering voters	.44	.30	.24	.32
Taking initiative in recruiting candidates	2.69	2.46	2.70	2.56
Attending party meetings*	74.59	63.79	73.44	63.42
Talking to public officials	2.31	2.36	2.02	2.20
Electoral Ambition	2.17	1.75	1.85	1.42

Source: Computations from survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

[a] Other variables entered in the discriminant analysis but not appearing in the final list of predictor variables were: Incentives: job experience, opportunity to be influential in the community; Activities: contacting other party officials, recruiting, talking to public officials on behalf of others, having run for public office in the past.

*These variables indicate expressive functions and were reverse-coded so that higher values indicate instrumentality.

determined by multiplying the party variable's function coefficient by the individual's standardized (z) score on the party variable. The products for all party variable function coefficients and related individual z scores are summed to provide each individual with a discriminant function score. Because all party variables are scored so that instrumental values are higher than expressive values, instrumental raw scores result in positive z scores and expressive raw scores result in negative z scores. The positive or negative result of the calculation of each major part of an individual's discriminant score on Function I is displayed in Table 5, and on Function II in Table 6.

Table 5, based on those variables characterizing Function I, demonstrates that it is the Democrats who score on the expressive ends of the incentive of enjoyment of politics and routine-nonroutine organizational roles but on the instrumental ends of the incentives of business contacts and future office-seeking. The Republicans are the reverse image of the Democrats on these party variables. Looking at the mean scores for these party variables in Table 4, we can verify that the Democrats tend to have instrumental scores on the incentives of the chance to run for office and the chance to make business contacts. Democrats have expressive scores reflecting enjoyment of politics and expressive, that is, routine scores on organizational roles. Thus, the Democratic activists are more likely to emphasize material and solidary incentives than are the Republicans, while the Republicans adopt more nonroutine organizational roles than do the Democrats.

Table 6, based on those variables characterizing Function II, demonstrates that men score consistently on the instrumental ends of the party variables. Women score consistently on the expressive ends of the party variables. A glance at the mean scores in Table 4 verifies that women exhibit less electoral ambition than men, attend more meetings, do more telephoning and less canvassing. Women also are less motivated to work for the party because of the opportunity to make business contacts and are more likely to define their electoral roles as

Table 5. Contributions of Major Party Variables in Function I to Individual Discriminant Scores on Function I

Most Discriminating Party Variables	Sign of Discriminant Function Coefficient	Contribution to Individual's Discriminant Score: With Instrumental *z* Score (+)	Contribution to Individual's Discriminant Score: With Expressive *z* Score (–)
Incentive: Enjoying politics	+	+	–
Role: Defining role as routine-nonroutine organizational	+	+	–
Incentive: Desiring to make business contacts	–	–	+
Incentive: Having chance to run for office	–	–	+

Source: Summarized from computations of survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

routine.

Tables 5 and 6 show that the expressive/instrumental concepts consistently distinguish between the sexes but not between the parties. The Democrats and the Republicans, when considered simply as party members, exhibit a combination of expressive and instrumental incentives and party role definitions.

The discriminant functions, of course, do not derive perfect divisions between the sexes on either function. Thus, on Function I, which divides the parties, Democratic women and Republican men are at the extremes with the means of Democratic men and Republican women falling to the inside of the extreme positions. On Function II, which divides the sexes, the extreme group scores are those of the Republican women and the Democratic men, while Democratic women and Republican men occupy the center portion of the array.

The relative positions of men and women on the two functions allow an appraisal of the findings of Jennings and Thomas (1968, pp. 483–84) that differences between the sexes are of less magnitude in the Democratic party than in the Republican party. The data employed in this study produce mixed findings. On the one hand, differences between the sexes are similar within both parties for discriminant Function II. The group centroids in Table 2, when compared between sexes within each party, show that on the average these differences are nearly the same within each party (a difference of .91 for Democrats and of .93 for Republicans). At the same time, Democratic women are more instrumental than Republican women, as are Democratic men in comparison to Republican men (differences of .52 for the women and .50 for the men). In other words, the level of instrumentality for both sexes is higher in the Democratic party, but the magnitude of differences between the sexes is similar in both parties. This pattern is especially marked for electoral ambition, the variable contributing most to distinguishing between the sexes (note the group mean scores for electoral ambition in Table 4). On the other hand, differences between the sexes are not similar within each party for discriminant Function I distinguishing the parties. The group means in Table 1,

Table 6. Contributions of Major Party Variables in Function II to Individual Discriminant Scores on Function II

Most Discriminating Party Variables	Sign of Discriminant Function Coefficient	Contribution to Individual's Discriminant Score: With Instrumental *z* Score (+)	Contribution to Individual's Discriminant Score: With Expressive *z* Score (–)
Ambition: Expressing aspirations for office	+	+	–
Activity: Attending party meetings	+	+	–
Activity: Telephoning	+	+	–
Incentive: Desiring to make business contacts	+	+	–
Activity: Canvassing	+	+	–
Role: Defining role as routine-nonroutine electoral	+	+	–

Source: Summarized from computations of survey data in the Fowlkes-Perkins Study of Atlanta Area Party Members, January-April, 1977.

when compared within each party, show that on the average, differences between the sexes concerning this function are somewhat greater for the Democrats (a difference of .70 between Democratic men and women) than for the Republicans (a difference of .50 between Republican men and women). Democratic women have the negative group means most removed from all the other groups largely because of their expressive position on the enjoyment of politics, the variable most responsible for distinguishing between the parties (see the group mean scores for enjoyment of politics in Table 4).

Discussion

As in other organizations, many aspects of life within the political party are gender-related in that men and women are differently employed. This study specifies and measures four aspects of party organization: party role definitions, party duties or activities, incentives for participation and electoral ambition. The evidence presented here suggests that in these party organizations gender distinctions are particularly evident in two areas: ambition and activities. Activities such as attending meetings and telephoning can be understood as fulfilling the expressive support function and more commonly as "women's work." More importantly, the party's major goal—that of recruiting candidates and capturing office—is also gender-related. According to our research, men are more ambitious than women, an expressive/instrumental differential that comports well with recent evidence that the under-representation of women in elective office is more the result of a paucity of women candidates than discrimination against them at the polls (Karnig and Walter, 1976; Darcy and Schramm, 1977).

What are the sources of sex-typed behavior? What are the implications for the party organization of change in gender roles? Possible sources of gender discrimination are early socialization and adherence to stereotypes because of fear of sanctions against deviant behavior (Lynn and Flora, 1977). And, of course, discrimination on the part of the party elite also is a possible source. However, as change occurs in society and challenges are carried out by women in party organization (Saloma and Sontag, 1973, pp. 108–10), one would expect that fewer women would be available for the routine functions they have performed in the past. Women may well be accommodated by being given a share of the more important positions, but will men begin to share in carrying out the very important expressive functions at the base of the party? As women adopt less traditional gender roles and become less responsible for performing their traditional party roles, and if men cannot be encouraged to do the same, the party organization, which is already weak because of other reasons, may become weaker.

The whole organizational life of the party, however, cannot be understood in terms of gender distinctions. The parties face common requirements in different ways, and the manner in which they meet common needs can be more a function of interparty variation than of gender differences. In two of the areas investigated—party role definitions and incentives for participation—party rather than sex is the more important variable. With respect to party role perceptions, Republican men *and* women see their organizational work as innovative, while Democratic women *and* men see their organizational roles as routine. And, while it is true that within both parties women are somewhat more motivated than men to work because they enjoy politics, the larger distinction is between the parties; both Democratic women *and* men are more expressive than their Republican counterparts. Similarly, women have less motivation to work for material rewards than men, but the important distinction is between the parties, with the Democrats the more instrumental of the two groups of activists. Even on those matters that are primarily gender-related such as political ambition, the Democratic party contains higher levels of ambition than does the Republican party.

The differences between the parties in the combination of instrumental and expressive incentives and party role definitions perhaps suggest something about the party system under investigation. The Democratic party is not only dominant in terms of current election returns but is also the long-standing party of the South. Those seeking material opportunity and the pleasure of politics may well find them in larger supply in the established party. Contrary to what some might expect of the minority party (Jewell and Olson, 1978, p. 74), the Republicans talk less about organizational roles and more about electoral roles than the Democrats. However, the organizational roles reported by Republicans are more innovative than those reported by Democrats, suggesting that the newer party necessarily must pursue organizational development while the established party is more concerned with routine maintenance functions.

The effects of party context may be unique to this particular setting, but the results of our study do encourage a closer and more general examination of the political structures in which men and women operate. The actual mechanisms by which party context affects organizational orientations and behavior are yet to be determined. The parties may attract people of different backgrounds and expectations, or the organizational setting and its political environment may mold and shape party recruits. It is highly probable that both processes are at work. The evidence at hand encourages a research concentration on the comparative entry characteristics of women and men who become involved in party politics and how these characteristics are modified or reinforced by organizational context.

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An Alternative Conceptualization of Political Tolerance: Illusory Increases 1950s-1970s*

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This article proposes an alternative conceptualization of political tolerance, a new measurement strategy consistent with that conceptualization, and some new findings based upon this measurement strategy. Briefly put, we argue that tolerance presumes a political objection to a group or to an idea, and if such an objection does not arise, neither does the problem of tolerance. Working from this understanding, we argue that previous efforts to measure tolerance have failed because they have asked respondents about groups preselected by the investigators. Those groups selected as points of reference in measuring tolerance have generally been of a leftist persuasion. Our measurement strategy allowed respondents themselves to select a political group to which they were strongly opposed. They were then asked a series of questions testing the extent to which they were prepared to extend procedural claims to these self-selected targets. Using this approach, we found little change between the 1950s and the 1970s in levels of tolerance in the United States, a result that contradicts much recent research on the problem.

Many theorists have argued that although a democratic regime may be divided by fierce conflicts, it can remain stable if citizens remain attached to democratic or constitutional procedures and maintain a willingness to apply such procedures—the right to speak, to publish, to run for office—on an equal basis to all, even to those who challenge its way of life. In this instrumental sense, tolerance is understood as valuable because it helps to maintain a stable democratic regime. In addition, since a tolerant regime is generally thought to be a good regime, tolerance is sometimes understood as a good in itself, as an essential characteristic of the good society.

The earliest empirical studies of tolerance conducted during the 1950s (Stouffer, 1955; Prothro and Grigg, 1960; McClosky, 1964) found high levels of intolerance and a good deal of unwillingness to extend civil liberties to objectionable groups. Many have therefore taken heart in recent findings which purport to show that levels of tolerance in the American public have increased substantially since these earlier studies were conducted (Davis, 1976; Nunn et al., 1978). It would appear that the political ferment of the 1960s and the declining salience of the cold war and of the communist issue have contributed to a more tolerant climate for political debate and dissent. According to this research, then, much progress has been made in the United States over the past two decades in building a more tolerant political regime.

However, the apparent connection between the social and political trends of the 1960s and 1970s and the changing levels of tolerance reported in these studies may dissolve upon closer inspection. Though domestic communists declined in salience and visibility during this period relative to the 1950s, they were replaced as potential targets of tolerance by other groups challenging the political consensus. These groups, representing all shades of political opinion, were not generally received in a tolerant manner, either by members of the elite or by the public at large. The claim that a changed climate of opinion produced higher levels of tolerance is thus too facile, and it begs a number of questions about the sources of

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tolerance and its meaning in a democratic regime.

These conclusions about changing levels of tolerance in American society derive from a particular tradition of research. This tradition incorporates common assumptions about the meaning of tolerance and about the way the concept should be measured. These assumptions, in turn, contributed heavily to the conclusions that were reached. As will be shown below, these assumptions about the meaning and measurement of tolerance are unwarranted and, hence, the conclusions based upon them are inaccurate or, at least, incomplete.

This article, therefore, will reconsider the problem from the beginning. Since the tradition of empirical research on tolerance is powerful and persuasive, it will be necessary to examine its assumptions in some detail to demonstrate our objections to it. After reformulating the problem on the basis of more tenable assumptions, we will proceed to our own analysis of the level of political tolerance in the United States. This analysis, in turn, will produce conclusions different from those of the studies cited above.

The Empirical Literature on Tolerance

Despite the importance of the subject, the literature on political tolerance is not particularly extensive. We can point to six important studies spanning two decades: Stouffer's (1955) study of attitudes toward communism; the Prothro-Grigg (1960) study of political tolerance; McClosky's (1964) study of levels of support for democratic norms; Lawrence's (1976) study connecting tolerance with positions on specific issues; Davis' (1975) study testing Stouffer's predictions about the effects of generation, age, and education on tolerance toward communists and atheists; and the Nunn, Crockett, and Williams (1978) more extensive update of Stouffer's work.

These studies are especially worthy of attention because they draw some important conclusions about the way in which democracy in the United States supposedly operates. As their conclusions are based upon empirical findings, close attention should be given to the way the studies are executed and to the evidence upon which these conclusions rest. In particular, they should be examined in light of the following questions: How is tolerance defined? How is it measured? And what assumptions are made concerning the role of tolerance in a democratic polity?

Among the earliest empirical studies of tolerance in the United States was Stouffer's analysis of public attitudes toward communism, published in 1955. Based upon a large national survey conducted in 1954, the study was designed to measure public attitudes toward communism and the extent to which Americans were prepared to extend procedural rights to communists and suspected communists. Stouffer also examined attitudes toward two other groups on the left, atheists and socialists. Though the study purported to study tolerance of "nonconformity," all but four of the fifteen items used to measure tolerance listed communists or suspected communists as points of reference (Stouffer, 1955, Appendix C). The conclusions of the study, therefore, bear more closely upon tolerance of communists than upon tolerance more broadly understood.

Stouffer's findings were nevertheless important—and disturbing. Substantial majorities said that an admitted communist should not be permitted to speak publicly, or to teach in high schools or colleges, or, indeed, to work as a clerk in a store. Majorities also agreed that communists should have their citizenship revoked, that books written by communists should be taken out of public libraries, that the government should have the authority to tap personal telephone conversations to acquire evidence against communists, and that, withal, admitted communists should be thrown in jail (Stouffer, 1955, Ch. 2). These attitudes softened considerably when the same questions were posed about socialists, atheists, and suspected communists. However, large numbers of citizens responded intolerantly to these targets as well. These results have to be interpreted against the background of the McCarthy period, but they undermined the assumption that there existed a consensus in the society around procedural norms that allow extremist groups access to political institutions.

Stouffer's conclusions about tolerance in the United States, however, were more optimistic than his empirical findings at first glance seemed to warrant. He suggested that tolerant norms in the society would inevitably grow stronger as time passed:

> Great social, economic, and technological forces are operating slowly and imperceptibly on the side of spreading tolerance. The rising level of education and the accompanying decline in authoritarian childrearing practices increase independence of thought and respect for others whose ideas are different. The increasing geographical movement of people has a similar consequence, as well as the vicarious experiences supplied by the magic of our even more powerful media of communications (p. 236).

Stouffer concluded, then, that the intolerance of the early 1950s would abate because of increased education and other factors.

Prothro and Grigg (1960) tried to discover whether there existed a consensus on general procedural norms of democracy and minority rights, and whether citizens were prepared to apply these abstract principles to specific situations in which unpopular groups of individuals might be involved. They did find a general consensus on the principles, but this broke down on the specific applications of the norms, particularly when the principles were applied to communists.

In a related study, McClosky (1964) compared political influentials and rank and file citizens in levels of support for abstract principles and for the application of these principles to specific situations. He found members of the elite to be much more sympathetic to statements expressing the "rules of the game"; in addition, they were more likely than the general electorate to support the application of the general principles of free speech and opinion to specific situations. He therefore concluded that "a large proportion of the electorate has failed to grasp certain of the underlying ideas and principles on which the American political system rests."[1]

These studies were carried out and written during the peak of the cold war era when the denial of procedural rights to communists and related groups was the major concern of those interested in civil liberties. In the interim, the dimensions of political conflict have grown more complex, and challenges to the political consensus have come from many sources, including civil rights activists, feminists, opponents of the war in Vietnam, and various radicals and reactionaries. As the potential targets of intolerance have proliferated, it is even less appropriate now to measure tolerance solely with reference to communists and associated groups. At the same time, by broadening the range of political opinion in the society, the ferment of the 1960s may have created a more tolerant environment for dissent. For these reasons, the conclusions of these earlier studies need to be reconsidered.

In a recent article, using National Opinion Research Center data collected in the early 1970s, Lawrence (1976) has reconsidered the problem and has refined the conclusions of these earlier studies. Like Prothro and Grigg, he is concerned with the relationship between positions on specific issues and the willingness to tolerate the actions of various groups concerned with these issues. Lawrence's findings report a higher level of tolerance in the 1970s than that suggested by earlier studies. A majority of citizens would permit all of the general acts of protest, except that involving the blocking of a government building, and there was considerable consistency between evaluations of the abstract acts and their specific applications. This consistency, however, varied from issue to issue.

The question of whether tolerance has in fact increased since Stouffer's study has been addressed in two separate studies. Davis (1975) attempted to test Stouffer's prediction that tolerance would increase as levels of education in the society increased and as the average age of the population declined. Drawing upon the same NORC sample Lawrence used, Davis found a 22 percent increase in tolerance between 1954 and 1971. He attributed 4 percent of this increase to higher levels of education, 5 percent to cohort replacement, and 13 percent to increasing levels of tolerance among all cohort and educational groups. Thus, the bulk of the change apparently reflected general trends that strengthened tolerant political norms. What these trends were he did not venture to say, though he suggested that they involved a "general movement" in the society toward more liberal positions on all sorts of non-economic issues.

Similarly, Nunn, Crockett, and Williams (1978) have attempted to measure changes in levels of tolerance by analyzing their own national survey conducted in 1973 that repeated the Stouffer items. Like Lawrence and Davis, they found a considerable increase in tolerance between 1954 and the early 1970s. Since their survey contained the same questions Stouffer used, they were able to measure quite precisely the changes in levels of tolerance of communists, atheists, and socialists. This is demonstrated in Table 1, where, using NORC data, we present the responses to six questions about communists and atheists in both 1954 and 1977. It is clear from these data that tolerance for these two groups increased significantly over this 23-year span. The increases range from 25 to 35 percentage points, depending upon the question. Nunn et al., relying upon data from their own survey and from several NORC surveys conducted in the early 1970s, observed a similar change. Thus, they found that while in 1954 only 31 percent of the public could be classified as tolerant on their overall tolerance index, fully 55 percent

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[1] Jackman (1972) shows that most of the differences between elite and mass disappear once education is controlled.

Table 1. Increases in Tolerance of Atheists and Communists, 1954–1977 (Percent)

	1977 NORC	1954 Stouffer	Increase
Should an atheist be allowed to speak?*	63	37	26
Should an atheist be allowed to teach?	39	12	27
Should a book written by an atheist be removed from the library?	60	35	25
Should a communist be allowed to speak?	57	27	30
Should a communist be allowed to teach?	41	6	35
Should a book written by a communist be removed from the library?	57	27	30

Source: Center for Political Studies, University of Michigan, 1954 data; National Opinion Research Center, University of Chicago, 1977 data.

*The data reported are the percentages giving the tolerant response. These questions have been repeated by NORC several times in the 1970s and the results are all virtually identical to those in column one above.

could be so classified by 1973. They concluded, "The most important finding from our efforts to track trends in American tolerance is that citizens who are most supportive of civil liberties have emerged as the majority in our society—and they are not a 'silent majority' " (p. 12).

The conclusions of these two studies then are, first, that levels of tolerance in the United States have increased significantly in the last 20 years and, second, that these changes flow partially from higher levels of education, partially from aging and cohort replacement, but primarily from broader political forces that have improved the climate for tolerance and civil liberties. Nunn and his associates mention such factors as the secularization of society and the Watergate episode as sources of the change, and they go so far to conclude that, "given the substantial increase in public support for democratic principles, the risk of demagogic takeover or the undermining of civil liberties, is less now than it once was" (p. 158). In other words, they have gone well beyond their empirical finding that tolerance of communists, atheists, and socialists has increased and have proceeded to conclude that tolerant beliefs in general are now more widely held, and that support for democratic principles has increased as well. Furthermore, the conclusion that tolerance has increased since 1954 rests upon assumptions about the meaning and measurement of tolerance that Stouffer first introduced. The conclusion therefore hinges in part on the validity of these assumptions. To challenge this conclusion, we must reconsider the assumptions upon which it is based. As we do this, a more tenable basis for the study of tolerance will be developed.

A Reconsideration of Political Tolerance

Tolerance implies a willingness to "put up with" those things that one rejects. Politically, it implies a willingness to permit the expression of those ideas or interests that one opposes. A tolerant regime, then, like a tolerant individual, is one that allows a wide berth to those ideas that challenge its way of life (see Crick, 1973, Ch. 3).

We should observe at the outset, then, that tolerance presumes opposition or disagreement. If there is no reason to oppose, then there is no occasion for one to be tolerant or intolerant. The question does not arise, since it is pointless to ask people to tolerate a doctrine or practice of which they approve or toward which they are indifferent. The problem of tolerance only arises once there are grounds for real disagreement; what one proceeds to do at this point determines whether one is tolerant.

In this sense, tolerance is conceptually "content-free" in that the content of the ideas that one opposes are irrelevant to the principle itself. One is tolerant to the extent one is prepared to extend freedoms to those whose ideas one rejects, whatever these might be. The analytical problem, from our standpoint, arises from the fact that people oppose or reject different groups or ideas. Smith may be particularly concerned about communists, while Jones is concerned about the Ku Klux Klan, and so on. To measure tolerance, we must first discern an objection, and this cannot be assumed by simply asking respondents about generally unpopular groups. They must identify such groups for themselves, since the targets of intolerance may vary widely among individuals. If respondents are simply asked about groups that are generally assumed to be unpopular and tolerance is measured with reference to attitudes about these groups, we are bound to confuse tolerance with the contents of respondents' beliefs about the groups the investigator selects.

In the studies previously reviewed, the measures of tolerance were not content-free, since the questions asked invariably referred to specific groups, generally of a leftist persuasion. The items used in Stouffer's study, and in subsequent attempts to monitor changing levels of tolerance, referred to communists, atheists, and socialists. Hence, in these studies, tolerance and intolerance for these particular groups have been confused with tolerance and intolerance more generally understood. The difficulty with this technique is that while one might be tolerant of communists or other radical groups, one might at the same time be quite intolerant of other groups on the right, such as racists, fascists, or nativists. At the same time, those who support repression against groups on the extreme left could be very tolerant of the groups just mentioned. Thus, the validity of items used to measure tolerance is fundamentally called into question.[2]

A Content-Controlled Measure of Tolerance

What is needed is a measurement procedure which allows respondents themselves to specify the groups they most strongly oppose. In an attempt to obtain such a measure, we developed and tested the following measurement approach. First, we provided each interview respondent with a list of potentially unpopular groups that ranged from communists and socialists on the left, to fascists, John Birch Society members, and Ku Klux Klan members on the right. We also included a number of groups, such as atheists, pro-abortionists, and anti-abortionists, which we expected in some ways to represent positions that are orthogonal to the left-right dimensions. (See Appendix A for the exact question wording and the specific groups listed.) Respondents were then asked to identify the group they liked the *least,* and we made it very clear that they could select a group *not* on our list. Respondents were then presented with a series of statements in an agree-disagree format which elicited their views about a range of activities in which members of that group might participate. The following statements were among those included in the series:

(1) Members of the _____ should be banned from being president of the United States.

(2) Members of the _____ should be allowed to teach in the public schools.

(3) The _____ should be outlawed.

(4) Members of the _____ should be allowed to make a speech in this city.

(5) The _____ should have their phones tapped by our government.

(6) The _____ should be allowed to hold public rallies in our city.

The statements were read as they appear above with the blanks filled with the group selected by each respondent. Respondents were also asked to pick their *second* least-liked group, and questions 1–6 were repeated. (Additional groups could be volunteered by the respondent; in fact, we encouraged this. See Appendix A.)

Our intention was to avoid contaminating the tolerance-intolerance dimension with the respondents' political beliefs. If we had merely asked all respondents whether communists should be allowed to hold public office, their responses would depend not only on their levels of tolerance, but also on their feelings toward communists. Previous studies have looked at tolerance in either of two ways: (1) by asking respondents whether they would tolerate certain groups, such as communists, socialists or atheists (Stouffer, 1955; Davis, 1975); or (2) by asking respondents whether they agree with general procedural norms in abstract terms. (Prothro and Grigg, 1960, and McClosky, 1964, used both of these procedures.) The advantage of our procedure is that it creates a situation in which the evaluation of each respondent toward the group in question is held constant. This measurement generates "content-controlled" responses and also prevents respondents from expressing agreement with general norms which they then fail to apply to specific groups. Clearly, our measures are not "content-free" since there *is* a context and a specific group toward which each respondent must react. We thus call it a "content-controlled" measure, to emphasize that we have attempted to "control for" the content by allowing respondents to select functionally equivalent groups. We view the group selected by each respondent as having a similar meaning across respondents' psychic organizations, even though different groups are selected.

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[2] In order to examine the validity of our critique by using the Stouffer items, we have performed a maximum likelihood factor analysis of the 1954 and 1974 Stouffer items, applying the SIFASP computer program which allows a simultaneous fitting of a model to separate populations (Jöreskog, 1969, 1970, and 1973). For a brief presentation of these results, see Sullivan et al. (1979). In brief, we find that almost all of the change in tolerance in the Stouffer items can be explained as a result of the decreasing salience of the three groups studied: communists, socialists, and atheists. Almost none of it can be accounted for by changes in attitudes toward the three acts noted in Table 1 (allowing atheists or communists to speak, to teach, or to have their books in a public library). A somewhat more detailed analysis of these data can be obtained from the authors. Our case, however, rests primarily upon the original data presented in this article.

A second sample was presented with the Stouffer approach to measuring tolerance, which specified either communists or atheists as points of reference. These items are identical to those used by Stouffer in his 1955 study (and by NORC in the 1970s) to measure tolerance of nonconformity, and they consisted of the following questions:

(1) There are always some people whose ideas are considered bad or dangerous by other people. For instance, somebody who is against all churches and religion. If such a person wanted to make a speech in your city against churches and religion, should he be allowed to speak, or not?

(2) Should such a person be allowed to teach in a college or university, or not?

(3) If some people in your community suggested that a book he wrote against churches and religion should be taken out of your public library, would you favor removing this book, or not?

(4) Now, I should like to ask you some questions about a man who admits he is a communist. Suppose this admitted communist wanted to make a speech in your community. Should he be allowed to speak, or not?

(5) Suppose he is teaching in a college. Should he be fired, or not?

(6) Suppose he wrote a book which is in your public library. Somebody in your community suggests that the book should be removed from the library. Would you favor removing it, or not?

By our standards, these questions do not guarantee equivalence of group meaning, since they measure tolerance with reference to groups selected by the investigators. We would expect these items primarily to measure attitudes toward communists and atheists rather than tolerance. Since we have presented these different sets of questions to separate random samples, we can readily measure the influence of different measurement strategies.

Levels of Tolerance: Differences by Measurement Strategy

We conducted two independent surveys in Minneapolis and St. Paul, Minnesota during the spring and summer of 1976. One independent random sample of respondents was asked our new set of tolerance questions while the other was asked the old Stouffer set. Two independent random samples of size 300 were selected from the Twin Cities' city directories, which include a listing of all adults in Minneapolis and St. Paul. Interviews were completed with 200 persons using the old questions and with 198 persons using the new questions, a response rate of about 66 percent.[3]

In addition, we will present data from a national opinion survey conducted for us by NORC in the spring of 1978. We included our content-controlled questions as well as four of the Stouffer questions. Thus we can compare the results both from an equivalent samples design and from a single sample which was asked both sets of questions.

Table 2 presents the percentage of tolerant responses for questions using both the least-liked and the second least-liked groups in one of the Twin Cities samples. The comparable

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[3] Interviewers were trained by the senior author during a one-day workshop and subsequent individual training sessions. Each interviewer conducted several practice interviews and was evaluated by the senior author and the subjects being interviewed. A lengthy training manual was prepared and much time was spent to ensure that interviewers would handle similar situations and problems in the same, objective manner. Weekly meetings were held after the interviewing began to go over problems and to standardize responses to these problems. The interviewers were hired through the University Employment Service at the University of Minnesota. Approximately 15 interviewers were used, most of them students. All interviewees were called or visited personally by a research assistant subsequent to the interview to ensure that the reported interview had taken place and that the interviewer had been competent. We have been unable to discover any data problems related to interviewers.

The two samples do not differ from each other or from the population on any demographic characteristics (see Sullivan, Piereson and Marcus, 1978). Both samples are representative of the larger population, and differences in tolerance may thus be attributed to different measurement strategies rather than differences between samples per se.

Table 2. Levels of Tolerance for Content-Controlled and for Stouffer Items

	1976 Twin Cities Split Samples		1978 NORC
A. Content-Controlled Items	Percent Tolerant, Least-Liked Group	Percent Tolerant, Second Least-Liked Group	Percent Tolerant, Least-Liked Group
Members of the ______ should be banned from being president of the U.S.	28	32	16
Members of the ______ should be allowed to teach in public schools.	26	30	19
The ______ should be outlawed.	33	43	29
Members of the ______ should be allowed to make a speech in this city.	70	67	50
The ______ should have their phones tapped by our government.	70	70	59
The ______ should be allowed to hold public rallies in our city.	57	51	34
B. Stouffer Items	Percent Tolerant		Percent Tolerant
Should an atheist be allowed to speak?*	78		65
Should an atheist be allowed to teach?	63		40**
Should a book written by an atheist be removed from the library?	80		62
Should a communist be allowed to speak?	71		63
Should a communist be allowed to teach?	62		40**
Should a book written by a communist be removed from the library?	79		64

Source: Twin Cities Survey and National Opinion Research Center, University of Chicago.

*See the text for the exact question wording. For part A, N = 198 and for part B, N = 200, in the 1976 split samples.

**Question not asked in the 1978 survey. Figures are from the 1977 NORC General Social Survey. N = 1509, in the 1978 NORC Survey.

percentages for the Stouffer items, given to the other Twin Cities sample, are also in Table 2, as well as the results from the 1978 NORC survey. According to the Twin Cities sample which responded to the Stouffer items, strong majorities are tolerant on every item, ranging from 62 percent tolerant on the "communist-teach" question to 80 percent tolerant on the "atheist-book" question. According to the other Twin Cities sample, which responded to the content-controlled questions, the portrait looks more bleak. The percentage tolerant ranges from 26 on the teaching question, to 70 on the speech and wiretapping questions. In fact, one finds only one-third of the citizenry willing to say that their least-liked group ought not to be outlawed. Further, only about one-fourth of the respondents were willing to allow members of their least-liked group to teach in schools, and to say they ought *not* to be banned from being president. Only about half were willing to allow this group to hold public rallies in their cities.[4]

It is our interpretation that the content-controlled items reveal more intolerance than the Stouffer items because they allow respondents to select from a much wider range of groups. People who may be willing to allow communists and atheists their full range of civil liberties, perhaps because they sympathize with such groups or fail to find them particularly threatening, may be unwilling to allow the Ku Klux Klan the same rights, perhaps because they do not sympathize with the Klan and find it dangerous.

None of the Stouffer items in column one produces a majority in favor of depriving either of these groups of fundamental civil liberties. It should be noted, however, that these sets of questions are not *strictly* comparable. The contents of the questions are not identical, and in addition, the content-controlled items were presented in an *agree-disagree* format while the Stouffer items were presented in a *yes-no* format. Nevertheless, over half of the Twin Cities respondents given the content-controlled items believe that their least-liked group should be outlawed, hardly consistent with the recent conclusion that the mass public is increasingly tolerant.

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[4] Since our questions are five-point agree-disagree items, the percentage tolerant reflects those who agree or agree strongly (or disagree, depending upon the *direction* of the statement).

Our content-controlled question on allowing the least-liked group to teach is worded similarly to the Stouffer item on allowing atheists or communists to teach, and while only 26 percent of the content-controlled sample thought members of a least-liked group should be allowed to teach, fully 63 and 62 percent of the sample responding to the Stouffer items gave the tolerant response. Certainly this suggests that although tolerance of communists and atheists has increased, the overall extent of tolerance may not have changed much at all. Our analysis suggests that the explanation for Davis' and Nunn's findings could well be one Davis rejects: that the cold war and fervor of the 1950s produced a convenient outlet for intolerance against communists and their "fellow traveler" atheists. Since the number of targets (on both the right and left) for political intolerance multiplied during the 1960s and 1970s, and since the cold war fervor of the political elites waned considerably during this time, we might well expect more tolerant responses toward communists and atheists while, at the same time, other groups were becoming the major targets of this intolerant impulse. We suggest that the aggregate level of tolerance may not have increased very much, even though tolerance toward these particular groups has undeniably increased.

Table 2 also presents the results from the national survey. In general, the results are consistent with our question-wording experiment. In the national sample, using the content-controlled questions, we find that 19 percent are tolerant on the teaching question, while with the Stouffer teaching questions, we find 40 percent are tolerant (for atheists and for communists). Similarly, 50 percent are tolerant if we rely on the controlled question about freedom of speech, while 65 and 63 percent are tolerant if we use the Stouffer items. So it appears that whether one uses one sample, including both sets of questions, or uses equivalent samples, asking each sample one set of questions, the conclusion is inescapable: higher levels of tolerance are indicated with the Stouffer items. And although some of the content-controlled items indicate a majority of tolerant citizens, most do not.

Some of the differences between the two sets of items are the result of somewhat different wordings of the questions. Further, the degree of tolerance depends in part on the nature of the act to be tolerated. Some acts are "easier" to tolerate: for example, allowing someone to speak as against allowing someone to teach. Nevertheless, the wording of two of the items across the sets, the speaking and teaching items, are similar enough to attribute the observed differences in tolerance to differences in measurement strategies.

The responses on the second least-liked group are slightly more tolerant than on the least-liked group. Certainly this is true of the first three items in Table 2, those that show the smallest proportion of tolerant responses. The first two indicate an increase in tolerance of 4 percent, the third an increase of 10 percent. This suggests that for each respondent, as we move from the least-liked group toward groups that are assessed more neutrally, we find the indicated tolerance to be higher. Therefore the Stouffer items produce more apparent tolerance because the communists and atheists are not "least-liked" for many respondents in the current population, so that these respondents appear to be more tolerant.

We can examine this point in more detail by analyzing the data in Table 3. In the content-controlled survey, after we asked respondents our six tolerance questions about their least-liked group, each respondent then judged that group (on a seven-point scale) on the seven pairs of adjectives listed in Table 3. We also repeated this procedure for each respondent's second least-liked group. In the Twin Cities sample that contained the Stouffer items, we asked each respondent to judge communists and atheists on the same seven pairs of adjectives.

Looking first at the mean scores for the content-controlled items, we see that the three most extreme adjective pairs are *good-bad, democratic-undemocratic,* and *dangerous-safe.* The other items all have mean evaluations among the middle three categories. The respondents' least-liked groups are perceived as uniformly bad, undemocratic, and dangerous. As one would expect, the means are less negative, closer to the middle categories, for the second least-liked group. A different picture emerges, however, for the Stouffer items. For the sample given these items, almost all seven adjective pairs for *both* communists and atheists have means in the middle categories of 3, 4, and 5. For example, although the least-liked group has a mean of 6.14 on the *good-bad* dimension, that figure is 5.41 for communists and only 4.61 for atheists; while for the least-liked group the mean on *democratic-undemocratic* is 6.38, for communists it is 6.07 and for atheists it is 4.81. For the least-liked group it is 1.89 on *dangerous-safe,* for communists it is 2.62 and for atheists it is 3.37. In fact, when one compares the means for the least-liked group to the means for communists and atheists, in all 14 instances the means move from the less to the more desirable evaluation. Thus, atheists are perceived as less important, less dishonest, less bad, less unpredictable, less dangerous, less strong, and less undemocratic than the respondents' least-liked group. The same is true of communists.

We feel quite certain, then, that the mass public is still generally intolerant today. Tolerance of atheists and communists has increased primarily because they are now perceived as less threatening and dangerous than they were in the 1950s. Other groups are now more salient, and one must study a multiplicity of groups to study tolerance. The reader might note that in every case, the mean score for the second least-liked group is less "negative" than that of the least-liked group. Although the differences are generally small, they are consistent. We expect that if one were to progress down the line toward third, fourth, fifth, etc. least-liked groups, tolerance would slowly increase.

In the national sample, the mean score for the least-liked group was 1.97 on *dangerous-safe* and 6.09 on *good-bad.* Thus, we have again found groups toward which almost all respondents feel negative, and which almost all feel are dangerous. That, of course, is the intent of our measurement procedure. If some respondents feel more positive than others toward the group we ask them about, it is not clear that we are measuring tolerance in a meaningful way. We feel confident that our procedure controls for such exogenous influences.

The Content-Controlled Measure: A Brief Look at Validity

The exact question wording of our content-controlled measure is given in Appendix A. The frequency distributions for groups selected as least-liked and second least-liked are found in Table 4. (All data presented from this point on are from the NORC 1978 national survey.) Only 38 percent of all respondents selected the socialists, communists or atheists as their least-liked groups, which suggests that in the earlier NORC studies used by Davis (1975) and the study by Nunn et al. (1978), approximately 60 percent of respondents were probably questioned about other than their least-preferred group. In fact, fully 30 percent of our respondents selected one of the three radical-right groups included in our analysis, and an additional 14 percent chose the Black Panthers and the Symbionese Liberation Army, radical-left groups not in existence in the 1950s. The major point, of course, is that the choices were spread out across the ten groups; if we were to ask respondents about only a subset of them, it would be more difficult for some respondents

Table 3. Semantic Differential Items for Both the Content-Controlled and the Stouffer Measures: Twin Cities Samples

	Content-Controlled Measures		Stouffer Measures	
	Mean Score* for Least-Liked Group	Mean Score for Second-Least-Liked Group	Mean Score for Communists	Mean Score for Atheists
Important-Unimportant	4.55	4.29	4.33	4.48
Honest-Dishonest	5.27	5.20	4.77	3.74
Good-Bad	6.14	6.09	5.41	4.61
Predictable-Unpredictable	4.96	4.66	4.71	4.65
Dangerous-Safe	1.89	2.20	2.62	3.37
Strong-Weak	3.62	3.48	3.63	3.92
Democratic-Undemocratic	6.38	6.24	6.07	4.81

Source: Twin Cities Survey, 1976.

*These means are based on semantic differential scales ranging from 1–7. The higher the mean, the closer the average perception was to the second of the adjective pairs. For example, the mean of 6.14 for *good-bad* reflects the fact that almost every respondent perceived their least-favorite group as *bad,* while the mean of 1.89 on *dangerous-safe* means almost all of them perceived this group as *dangerous* (rather than *safe*).

Table 4. Targets of Intolerance (Percent)

	Least-Liked Group	Second Least-Liked Group
Socialists	1	2
Communists	29	19
Atheists	8	7
Symbionese Liberation Army	8	10
Black Panthers	6	10
Pro-abortionists	4	5
Anti-abortionists	2	3
John Birch Society	1	3
Ku Klux Klan	24	14
Fascists	5	6
Other Group	2	1
Don't Know	10	19
N = 1509		

Source: National Opinion Research Center, University of Chicago, 1978 Survey.

to be tolerant. For the 29 percent of our respondents who selected communists as their least-liked group, it would be quite difficult for them to exhibit tolerance if we merely asked them questions about the communists. For the remaining 60 or 70 percent, however, it might be much easier because they do not fear the communists, or feel them dangerous or threatening. This is likely to be true no matter which group we study, so long as we do the selecting and force each respondent to react to our predetermined groups.

To examine the validity of our measurement, we gave our respondents the traditional seven-point liberal-conservative self-identification scale. In Figure 1, we present the mean score on this scale for respondents who picked each of the various groups as their least-liked group. For example, the respondents who selected the John Birch Society as their least-liked group had a mean score on the self-placement scale of 3.38, toward the liberal end; those who selected the socialists had a mean score of 4.47, toward the conservative end of the scale. As is evident from the rank-ordering in the figure, the groups are ordered roughly as one would expect if one assumes that liberals generally dislike right-wing groups and conservatives dislike left-wing groups. Four groups of respondents had mean scores on the liberal side of 4 (the neutral point): those who selected the Birch Society, fascists, the Klan, and anti-abortionists as their least-liked. Six groups had means on the conservative side of 4: those who selected the socialists, the SLA, communists, atheists, the Black Panthers, and pro-abortionists. The groups of respondents selecting the groups studied by Stouffer (and subsequently by Davis and Nunn) had means ranging between 4.29 and 4.47, clearly on the conservative side. Thus in those studies, it is probably true that respondents who identified themselves as conservatives had more difficulty giving tolerant responses about the communists, socialists, and atheists than did those who considered themselves liberals. To study tolerance correctly, one needs to include groups from the entire ideological spectrum, and apparently our procedure affords such coverage.[5]

It appears that our measure does what it should—it discriminates between liberals and conservatives, and it presents a number of groups from across the ideological spectrum. This makes it likely that most of our respon-

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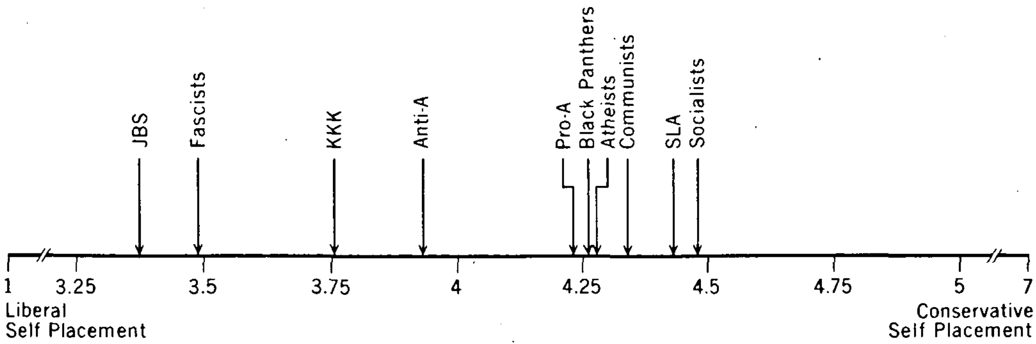
[5] To examine further the validity of our measurement, we repeated the analysis in Figure 1 using respondents' second least-liked group, and our results were identical. The extremes are again represented by those whose second least-liked group is the Birch Society (mean of 3.33) and those whose second least-liked group is the socialists (mean of 4.92). Again, respondents whose second least-liked group is the Birth Society, fascists, anti-abortionists, and the Klan, have mean self-placement scores on the liberal end of the continuum, while those whose second least-liked group is the Black Panthers, the SLA, communists, pro-abortionists, atheists, or socialists have mean scores on the conservative end of the continuum. We repeated this exercise using four of the Survey Research Center's issues questions—on jobs, medical care, school integration, and black welfare—and the results are basically the same. (We created a scale using these four issues, and did our analysis for both least-liked and second least-liked group. These figures are available from the authors.)



LEAST LIKED GROUP	MEAN SCORE, LIBERAL-CONSERVATIVE SELF PLACEMENT	
1. John Birch Society (JBS)	3.38	(N=21)
2. Fascists	3.49	(N=76)
3. Ku Klux Klan (KKK)	3.76	(N=335)
4. Anti-Abortionists (Anti-A)	3.93	(N=27)
5. Pro-Abortionists (Pro-A)	4.21	(N=53)
6. Black Panthers	4.26	(N=77)
7. Atheists	4.29	(N=112)
8. Communists	4.34	(N=396)
9. Symbionese Liberation Army (SLA)	4.43	(N=110)
10. Socialists	4.47	(N=19)

Source: National Opinion Research Center, University of Chicago, 1978 Survey.

Figure 1. Liberal-Conservative Self-Placement by Least-Liked Group

dents were able to find one of their disliked groups on our list.

Another way to examine the validity of our measurement procedure is to examine what we shall label "crossovers." By crossovers we refer to those respondents who select one right-wing and one left-wing group as their two least-liked groups. If our respondents do indeed select as disliked those groups furthest away from themselves on the ideological continuum, then we expect that respondents near the middle of the continuum would be more likely to be crossovers than those who more clearly label themselves as liberals or, conservatives. These results are presented in Table 5. Among people who define themselves as the most liberal on our seven-point scale, 40 percent of them select two right-wing groups as their two least-liked, despite the fact that there are only three groups that we can clearly label as right-wing. There is a sharp decline in the percent selecting two right-wing groups as we go across the categories of conservatism. At the other extreme, only one percent and zero percent of those who score themselves as 6 or 7 on the conservatism scale select two right-wing groups.

Table 5. Liberal-Conservative Self-Placement and Target Groups Picked (Percent)

	Liberal						Conservative
Groups Picked Are:	1	2	3	4	5	6	7
Both Left-Wing	11	16	20	35	38	47	53
One Left and One Center*	0	6	9	14	10	12	6
One Left and One Right	40	49	53	41	42	38	29
One Right and One Center	9	9	5	6	5	2	12
Both Right-Wing	40	18	13	4	4	1	0
N =	(35)	(109)	(136)	(428)	(224)	(114)	(17)

Source: National Opinion Research Center, University of Chicago, 1978 Survey.

*The two abortion groups are defined as centrist groups. (No respondents selected both abortion groups as their two least-liked groups.) Note that in Figure 1, the two abortion groups are the two closest to 4, which is the midpoint on the liberal-conservative scale.

On the other hand, fully half of those who label themselves as conservatives select two left-wing groups, while only about one in ten of the liberals does so. Looking at the crossovers, we see that those respondents who place themselves toward the middle of the scale are in fact more likely to select one left- and one right-wing group. Going from left to right, 40 percent of liberals are crossovers, and this percentage rises to 53 percent for the third category, and then begins a decrease until only 29 percent of conservatives are crossovers. (More liberals are crossovers because there are five left-wing groups and only three right-wing groups on our list.) Although the pattern does not peak in category 4, it is clearly curvilinear, particularly in comparison to the straight linearity exhibited by the first and last rows in the table.

Concluding Comments

The foregoing analysis leads to two important conclusions about tolerance in the United States:

(1) Stouffer's method of measuring tolerance with reference to communists, socialists, and atheists is inadequate and, to a large extent, time-bound. It is inadequate because it does not fully capture the meaning of the concept of tolerance. It is time-bound because it presumes that these particular groups are the only important targets of intolerance in the society. This *may* have been more or less true in 1954, so that Stouffer's conclusions *may* have been appropriate for the limited purposes of his research. But it is certainly not true now. Hence, attempts to monitor changing levels of tolerance with this procedure are inappropriate and produce misleading conclusions.

(2) Substantively, the content-controlled method of studying tolerance developed above reveals that intolerance has not necessarily declined much over the past 25 years, but merely has been turned toward new targets. Our data show that while the mass public is now more tolerant of communists, atheists, and socialists than it was in 1954, other targets of intolerance have emerged in the meantime to neutralize this change. On the face of things, then, it appears that the present period differs from the earlier one in that there are now more targets of intolerance but none which is sufficiently important to generate a major threat to civil liberties.

These conclusions, while important in their own right, also raise other questions about the understanding of tolerance that has been handed down through the earlier tradition of research in the area. If we are correct in asserting that tolerance has been incorrectly conceived and measured, we expect that other accepted generalizations in the area might also be of dubious validity. For example, Stouffer and others found that, among individuals, education was the most important "cause" of a tolerant outlook. The work that we have done so far on this question suggests that this relationship is largely an artifact of the groups selected as points of reference against which to measure tolerance. Paradoxically, those with lower levels of education are most threatened by and most opposed to dissident groups on the left—that is, precisely those groups selected in the earlier studies as points of reference for measuring tolerance. When individuals are given the opportunity to select the groups (from both the left and the right) to which they are opposed, the powerful relationship between education and tolerance is reduced considerably. The faith, therefore, that many have placed in education, conventionally conceived, as a solution to the problem of intolerance is apparently misplaced.

It should be emphasized that we are not resurrecting the old argument, developed by Prothro and Grigg (1960) and McClosky (1964), that because large numbers of citizens are intolerant, a meaningful democratic politics, with all that this implies, is a utopian goal. There is a sense in which these writers began their studies with a utopian version of democracy, a major condition of which was that nearly all citizens would accept the creed of tolerance in a form similar to that laid down by John Stuart Mill in *On Liberty*. When citizens did not measure up to the standard, these scholars began to recast their understanding of democracy in order to find sources of democratic stability in places other than in citizen virtue. Now that more recent studies have found that levels of tolerance are on the increase, it has been suggested (Nunn et al., p. 159) that we can begin to resurrect the classical theory. In this strange way, empirical findings concerning levels of tolerance in the society have shaped our understanding of democracy itself.

As others (Berns, 1962; Pateman, 1970) have pointed out, the "classical" view of democracy which served as the theoretical compass for these studies was itself a modern construction. As such, the theory did not take into account a number of fundamental questions about the relationship between tolerance and democratic politics that were raised by earlier liberal thinkers. The theory assumes, for example, that political tolerance is good and that any deviation in the society or among individuals from an absolute standard of tolerance is undesirable. In this sense, it rests upon a normative view of democracy that resembles the position taken by Justice Oliver Wendell Holmes in his famous dissent in the case of *Abrams* v. *United States* (1919: 630):

> If you have no doubt of your premises or your power and want a certain result with all your heart you naturally express your wishes in law and sweep away all opposition. To allow opposition by speech seems to indicate that you think the speech impotent, as when a man says he has squared the circle, or that you do not care wholeheartedly for the result, or that you doubt either your power or your premises. But when men have realized that time has upset many fighting faiths, they come to believe even more than they believe in the very foundations of their own conduct that the ultimate good desired is better reached by free trade in ideas—that the best test of truth is the power of the thought to get itself accepted in the competition of the market, and that truth is the only ground upon which their wishes safely can be carried out. That at any rate is the theory of our Constitution.

This is not the place to raise the various criticisms of this doctrine, since most of them are well known. The merits of these abstract points aside, it seems to us that any theory of democracy that relies upon a widespread acceptance of this doctrine, or of something similar to it, is quite unrealistic and, in any case, unnecessary to the functioning of a democratic system.

It is more prudent, in our view, to take one's bearings on these questions from *The Federalist*. Contrary to Holmes, the Framers did not base the Constitution on the notion that political truth emerges from the competition of the market; nor did they believe that it was necessary that citizens accept this doctrine in order for a republican system to survive. As is well known, in *Federalist* 51 Madison (Cooke, 1961, pp. 351–52) put his faith in more practical and realistic safeguards:

> In a free government, the security for civil rights must be the same as for religious rights. It consists in the one case in the multiplicity of interests, and in the other, in the multiplicity of sects; and this may be presumed to depend on the extent of the country and number of people comprehended under the same government.

For Madison, then, the safeguards consist in the processes of politics and in the requirements of coalition rather than in the acceptance among citizens of an abstract creed similar to that suggested by Holmes. To be sure, Madison recognized that this was a problematic solution and not a hard and fast safeguard.

It makes some sense, therefore, to interpret the findings of this article in terms of Madison's prescriptions. When the political system provides a multitude of convenient targets for intolerance, the result is what one might call "pluralistic intolerance." The political consequences of such a situation may be quite different from those of a situation in which dominant targets of intolerance exist, a difference not unlike the varying consequences of cross-cutting versus overlapping cleavages. The findings of this study suggest that even though levels of intolerance are now quite high in American society, the diversity of the targets of intolerance prevent, for the time being, a substantial threat to civil liberties. Nevertheless, for those truly concerned with this problem, the dangers of intolerance still exist, for given the right circumstances, these attitudes could be focused and mobilized, as they were in the 1950s. For those who will escape into abstractions, this will seem a pessimistic conclusion. For others, perhaps, who are used to thinking about the realities of politics, it may come as no surprise.

Appendix A

Instructions to interviewer: Hand the respondent our handout A, the "List of Groups in Politics." Then say:

> I am giving you a list of groups in politics. As I read the list please follow along: socialists, fascists, communists, Ku Klux Klan, John Birch Society, Black Panthers, Symbionese Liberation Army, atheists, pro-abortionists, and anti-abortionists. Which of these groups do you like the least? If there is some group that you like even less than the groups listed here, please tell me the name of that group.

(Note to interviewer: If they have trouble making up their mind, encourage them to think, just generally, which group is the most unpleasant, in their opinion. If they really can't decide, mark that opinion below.)

____ respondent can't decide; doesn't know
____ respondent dislikes group not listed here (fill in name of group below)

____

____ socialists
____ fascists
____ communists
____ Ku Klux Klan
____ John Birch Society
____ Black Panthers
____ Symbionese Liberation Army
____ atheists
____ pro-abortionists
____ anti-abortionists (pro-lifers)

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Presidential Power versus Bureaucratic Intransigence: The Influence of the Nixon Administration on Welfare Policy*

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Many observers routinely assert the relative weakness of presidents before the bureaucracy. The research of this study, guided by a structuralist theory of organizations, provides evidence of the Nixon administration's power to change policy, even over the opposition of the bureaucracy, concerning the Aid to Families with Dependent Children program. The study demonstrates that the management tools available to the president and top officials, when used adroitly, are more powerful than are generally presumed. That presidents can affix their indelible stamps on policy by short-circuiting the legislative process and dominating the bureaucracy is more than a remote possibility.

Presidents and political scientists often agree on the extraordinary difficulties of exercising presidential direction and control over the bureaucracy. Many officials bore the wrath of Richard Nixon, such as the Small Business Administration agent who disobeyed orders: "[He] deliberately did not—I read the memorandum—he did not carry out an order I personally gave" (*New York Times*, 20 July 1974, p. 14). In an Associated Press interview after eight months in office, Jimmy Carter agreed that presidential powers were more limited than he expected: "I underestimated the inertia or the momentum of the federal bureaucracy. . . . It is difficult to change" (*New York Times*, 23 October 1977, p. 36). Long before Nixon and Carter, other presidents complained of the intransigence of the bureaucracy (Hughes, 1973, pp. 181–95).

From their own frustrating experience of concentrating attention on decisions not implemented, presidents understandably feel that the bureaucracy continually sabotages their programs. Presidents see more intransigence than compliance in the bureaucracy because they necessarily spend more time pursuing failures in direction and control than savoring successes.

Relying on the recollections of presidents, aides, and departmental officials about presidential-bureaucratic relations, many political scientists have come to the same conclusion about the difficulties of exercising presidential direction and control. For example, after interviewing 43 members of recent presidential staffs who recalled a high level of conflict with the departments and agencies, Cronin (1970, p. 391) concluded that "presidents have been and will continue to be frustrated by the sluggishness of the federal executive branch's response to new priorities."

The 1976 Aberbach and Rockman report, based on a 1970 survey of top federal career and political officials, asserted that bureaucrats are too Democratic and political appointees too liberal for a conservative Republican president to have much policy impact. Their prediction for the "forseeable future": "The White House will continue to reflect short-term political changes and . . . the bureaucracy will be *relatively immune* to these dynamics" (Aberbach and Rockman, 1976, p. 467, emphasis added).

Just as presidential recollections accentuate the weaknesses of the office and the powers of the bureaucracy to block their initiatives, a political science research strategy which focuses interview questions on presidential-bureaucratic conflict (rather than changes in policy accomplished because of presidential actions) magnifies the obstacles to presidential control. Shortly after the Aberbach-Rockman survey, President Nixon changed his approach, intending "to *take over* the bureaucracy and *take on* the Congress, to concentrate on administrative steps and correspondingly to downgrade legislation as the principal route for bringing about domestic policy change" (Nathan, 1975, p. 8). Many of the stormy presidential-bureaucratic battles that ensued produced a plethora of painful policy losses for the bureaucrats.

Rather than employ a research strategy that emphasizes the recollections of presidential conflict with bureaucrats, this study examines the means used by the Nixon administration to influence the Aid to Families with Dependent Children (AFDC) program, and offers an assessment of the subsequent changes. Although this approach prevents the inevitable presidential-

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*Martha Derthick, Jane S. Randall, and David S. Wilson provided helpful comments. Part of the research for this study was conducted while the author held a University of Toledo Summer Faculty Research Fellowship.

bureaucratic conflict from obscuring the very real powers available to administratively astute presidents, this study does draw on interviews with bureaucrats who are probably inclined to view their programs as especially vulnerable to political interference, inviting the criticism that one methodological bias has been substituted for another. However, the focus on the means actually employed to change the AFDC program and the discussion of the policy changes which followed should blunt that criticism. More importantly, this focus will demonstrate that under certain conditions the tools available to the president, when used adroitly, are more powerful than generally presumed.

The welfare bureaucracy of the Department of Health, Education and Welfare inherited by President Nixon in 1969 was part of a highly complex system that included the state and local welfare offices. For nearly 35 years, federal bureaucrats had been suggesting, guiding, prodding and otherwise encouraging the state and local organizations to be more professional, less restrictive in determining eligibility of claimants, and more generous in payment levels. Nixon's attitude toward welfare was decidedly more restrictive. The techniques his administration employed to implement his views beginning near the end of his first term will not surprise students of organizational theory, especially those of a structuralist persuasion (Perrow, 1972) who stress that a knowledgeable manager usually can find the resources to make significant policy changes despite opposition and conflict with subordinates. March and Simon (1958, p. 129) suggest four possible reactions to organizational conflict: problem solving, persuasion, bargaining, and "politics." A thoroughgoing structuralist approach, however, suggests that skillful managers achieve their greatest policy successes over the bureaucracy by manipulating structure, e.g., changing work assignments, the division of labor, the communication system, and influencing recruitment and promotion, training, and indoctrination. This occurs especially in large organizations, where budgets are big and where deft administrators have slack resources available for modifying the organizational structure to achieve their goals.

The assault on welfare began in 1971. It was manifested initially by an extraordinary change in welfare advisers and administrators. Although there may not have been a detailed blueprint at this time for taking over welfare, the new team responded to Nixon's policy views and cues by developing and refining managerial means to influence policy.

Changing the Nixon Welfare Administrators

Like many new presidents, Richard Nixon had only vague notions of the specific directions that he wanted domestic policy to take. Despite his years in Congress and the vice-presidency and his acquaintance with Republican party workers and officials around the nation, he knew surprisingly little about government in Washington. Staffing the highest levels of government required naming people with whom he was unacquainted (Evans and Novak, 1971, pp. 9–10, 66). Lacking a clear sense of purpose and specific policies, President Nixon allowed department heads to hire the lesser executives, an action he soon regretted.

When liberal Nixon protégé Robert Finch was appointed to head HEW, he staffed it with a group of progressives. Over the opposition of Governor Ronald Reagan of California, Finch selected John Veneman, as under-secretary, responsible for welfare policy. Veneman, a liberal California legislator, expert in welfare and health programs, had opposed Reagan's proposed cutbacks of Medicaid benefits in that state. Veneman brought in Tom Joe, who was largely responsible for exploiting federal regulations to allocate large amounts of federal money for increases in California social services (Burke and Burke, 1974, pp. 50–51; Derthick, 1975). In addition, Veneman surrounded himself with several holdover Democrats who had played important roles in welfare planning in the Johnson administration and gave them a key role in developing the Family Assistance Plan (Burke and Burke, 1974).

At lower levels of HEW the politically appointed welfare administrators also consisted of a surprisingly large number of holdover Democrats and/or welfare professionals. Mary Switzer remained from the Johnson administration as director of the Social and Rehabilitation Service (SRS), the principal HEW welfare unit. She was a professional regarded highly in Congress, especially for her work in the Vocational Rehabilitation Administration. Stephen Simonds, a dedicated welfare professional with an educational background in social services administration, remained as head of the Assistance Payment Administration (APA), the SRS unit most responsible for the AFDC program. In May, 1970 he moved to the directorship of the APA sister agency, the Community Services Administration, where he proved to be a friend of welfare recipients by promoting social services.

The new commissioner of the Assistance

Payments Administration was John L. Costa, also a welfare professional, educated in social work, who had a long record of experience ranging from case work to state and federal social work administration.

Not until April, 1970 was a Republican appointed to head the SRS. Like his successors, John Twiname talked a management line, but this was "more rhetoric than reality," according to Derthick (1975, p. 36), who says his policy "instincts were liberal." He was compassionate and brought little change in policy. In mid-1971, Twiname recruited James Bax as commissioner of the Community Services Administration. Bax was flamboyant, political, and unconstrained by conservative Republican welfare principles. Derthick (1975, p. 72) relates how the Bax promotional philosophy was captured by the aphorism that spread throughout HEW: "You hatch it, we match it."

After two and a half years of Republican control of the executive branch, the administration of welfare rested in the hands of individuals at least as tolerant toward welfare, probably more so, than those in the previous Democratic administration. During this period, a major overhaul of the welfare system—the Family Assistance Plan (FAP)—was thrice submitted to Congress, welfare rolls continued to expand, and social services grants to the states soared from $354 million in fiscal year 1969 to $1.6 billion in FY 1972, and would have reached $4.7 billion in FY 1973, had not the State and Local Fiscal Assistance Act of 1972 (86 Stat, 945–47) put a $2.5 billion ceiling on the expenditures (U.S. Budgets, FY 1971, p. 436; FY 1974, p. 442).

The Nixon approach to the presidency and to domestic policy was undergoing a metamorphosis in 1971. Frustrated by the first-term legislative strategy which yielded little success in the face of a Democratic Congress and by not "getting domestic agencies to do what was wanted" (Nathan, 1974, p. 72), President Nixon changed from a legislative to a managerial strategy to achieve his goals. Although initially he "seemed excited and pleased about what most would consider the progressive elements of his domestic program," the second-term shift in strategy was accompanied by a significant change in philosophy: a decided shift to the right, "to a more pessimistic and conservative position in the area of domestic affairs," according to Richard Nathan (1975, pp. 70, 72), who held high-level positions in the first term. However, the dropping of the Family Assistance Plan exaggerates the appearance of change in Nixon's welfare philosophy. His embrace of FAP was partly an aberration from his long-held welfare attitudes, attributable to the extraordinary persuasiveness of presidential advisor Daniel Patrick Moynihan (Burke and Burke, 1976). Mostly, his welfare reform support issued from his own understanding of FAP, which put more emphasis on work incentives than did most other observers (Nathan, 1973, p. 19). Thus, while supporting FAP, Nixon remained truer to his long-held welfare views than is generally perceived.

Whatever the change in Nixon's welfare attitudes, the approach of his administration toward welfare policy changed dramatically as the emphasis turned from helping the poor (albeit the working poor) to welfare cheats, reduction of social services, etc. President Nixon publicly affirmed this changed posture in a celebrated newspaper interview given to Garnett D. Horner on the Sunday before the 1972 election (published shortly afterwards) when he said:

> Another thing this election is about is whether we should move toward more massive handouts to people, making the people more and more dependent, or whether we say, no, it is up to you. The people are going to carry their share of the load (*Washington Star-News,* 9 September 1972).

President Nixon's determination to take charge of the welfare bureaucracy grew. In HEW, personnel changes occurred at all appointive levels. Robert Finch and John Veneman left. In SRS, John Twiname was eased out. Lower in the department, James Bax, Stephen Simonds, John Costa and others of similar view departed.

Caspar Weinberger became HEW Secretary on February 12, 1973. His protégé, James S. Dwight, Jr., was appointed head of SRS soon afterward. They had served in Governor Reagan's administration in California as director and chief deputy director of finance, respectively, where they vigorously implemented cutbacks in welfare (Derthick, 1975, p. 104). In 1970, Weinberger served a short stint as chairman of the Federal Trade Commission and proved himself a competent administrator. Later that year, he moved to the Office of Management and Budget to become deputy director and then director. Weinberger had a role in the Nixon vetoes of HEW appropriations, which had been routine since 1969, and came to HEW with the sobriquet "Cap the Knife."

Although a good cabinet officer generally is presumed to be an advocate for the department's programs, Weinberger had a political orientation considerably at odds with that usually associated with the activist disposition of HEW. Concerning the role of the department he led for more than two years, he lamented: "I am highly skeptical of the Federal government's ability to be a social engineer" (Weinberger, 1975, p. 22). In his last major speech as secretary, he decried the very programs that he administered, warning that expanding social programs threaten personal freedom and are likely to consume half the gross national product by the year 2000. "Should that day ever come, half of the American people will be working to support the other half" and "government would be like a gigantic sponge, sopping up all the nation's surplus capital needed for industrial growth and modernization" ([Toledo] *Blade,* 22 July 1975).

Dwight quickly gained his own sobriquet at HEW: "Attila the Hun." His efforts to implement a restrictive welfare policy at SRS were facilitated by his perceived proximity to power. As a bureaucrat in an HEW research unit expressed later:

> Most of the people [in SRS] went along with Dwight. He was the sun-god. He was close to Weinberger and Weinberger was next to the president. He was getting support from Weinberger and the White House. He was carrying out the kind of program they wanted (interview, July 1977).

Once entrenched, the new administrators found a number of ways to make national welfare policy more restrictive.

Management Resources to Change Welfare

According to the structuralist view of organizations, top administrators have diverse resources for exercising direction and control. After 1971, in reshaping welfare, the Nixon administration most successfully utilized a new monitoring and evaluation system and reorganization of the Social and Rehabilitation Service, including regionalization.

Monitoring and Evaluation. The new monitoring and evaluation system provided the Nixon administration with its strongest tool to reshape welfare. At its heart was a "quality-control" system of reviewing case worker actions for certain types of errors while totally ignoring others.

Before the 1970s, administrative reviews of case-work actions toward welfare applicants and recipients were handled on a case-action basis, whereby officials examined a sample of all decisions taken by welfare employees, including negative case actions (i.e., rejections of applications), for compliance with welfare laws and state regulations (as well as checking for the length of time required to determine eligibility, the time elapsed before the first payment, legality of terminating recipients, etc.). Quality-control reviews replaced these administrative reviews (which actually had fallen into disuse in the late 1960s).

The major impact of quality control depended on three developments in 1973: (1) establishment of tolerance limits (for maximal allowable error rates); (2) replacement of case-action surveys with a cross-section sample design; and (3) introduction of fiscal penalties.

Tolerance limits were set at 3 percent and 5 percent, respectively, for ineligibility and overpayment error rates for each of the states. Because of the complexity of welfare rules and regulations and the classification of small monetary discrepancies in payments as errors, all states far exceeded the tolerance levels in the first quality-control survey, with average error rates of 10.2 percent for ineligibility and 22.8 percent for overpayments (DHEW news release, 20 December 1973). Welfare workers considered the tolerance levels draconian, but knew they would succeed in forcing case workers in questionable situations to err by rejecting applications and making underpayments. The number of application denials and underpayments would inevitably climb.[1]

The switch from case-action to cross-section quality-control surveys plus the tolerance levels combined to intensify the pressure on intake workers to err on the restrictive side. With no examination of negative case action, quality control based on the cross section of active files provided no means to detect denials of eligible applicants. This insured that errors would become systemic and biased against those eligible for welfare. An official in a staff office of the HEW Secretary, who grudgingly admired the purposefulness of welfare policy makers, offered this analysis:

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[1] With strong anti-welfare sentiment in some parts of the country, many professionals worried about the excessive zeal in certain states that the federal emphasis would produce. Ohio, predictably, succumbed to the contagion of punitive quality-control efforts in the spring of 1975 by embarking on a 100-percent sample. The quality-control review required all Ohio recipients to appear at the county welfare offices with verification of earnings and other information. When some recipients failed to appear—2,279 out of 10,787 interviews scheduled in Lucas county ([Toledo] *Blade,* 7 July 1975)—state officials had their evidence to assert that a high proportion of chiselers were on the welfare rolls. Reasonable voices questioned the wisdom of a 100-percent sample, feeling that it was designed to purge the rolls of cases rather than produce genuine administrative improvements in the program. Of course, many of the no-shows were accounted for by the high turnover of welfare recipients regardless of quality-control surveys, and the number of recipients who were too ill or who lacked transportation and child-care to enable them to go to the welfare offices.

> SRS officials are very perceptive; they have goals, which are clear, and they are going to get them carried out. There are basically two types of errors: non-eligibles on the rolls and eligibles refused access to the roles. What SRS wants to do–and is doing–is drive down the first type of error. Because there is a trade-off between the two types of errors, they are increasing the other kind of error. SRS officials are forcing their will on a bureaucracy that would prefer to go another route (interview, March 1975).

Worried about the eligible persons denied welfare, many of the welfare professionals sought approval for methods to examine negative case actions during quality-control surveys. SRS policy makers showed no interest in providing this approval until December, 1976, when they were pressured by a federal judge presiding over a case filed by the National Welfare Rights Organization (*Federal Register,* 22 December 1976, to amend 45 CFR 205.25).

Fiscal penalties were also used to bring about changes in welfare policy, beginning on December 20, 1973, when Weinberger announced his intention to impose them for high error rates. Historically, the federal government has been reluctant to use penalties because they hurt the recipients more than the state administrators guilty of malfeasance (Altmeyer, 1966). Consequently, threats of fiscal penalties have been viewed more as invitations to negotiate differences than as non-negotiable demands for states to capitulate to federal ultimatums. But the states sensed the determination of the administration and were energized by the large amounts of money at stake. An HEW news release (16 December 1976) shows disallowances amounting to 3.7 percent of the total federal participation for one six-month period. For Washington, D.C., threatened withholding exceeded 15 percent. Taking advantage of the strong position granted them by the federal system to influence national policy, the states chose the courts over the bargaining table. Eleven states joined in a lawsuit filed in the federal district court in Washington, D.C. to enjoin the department and secretary from enforcing the disallowances (*State of Maryland et al.* v. *F. David Mathews et al.,* Civil Action No. 75–63). Concomitantly, other states filed similar suits in federal district courts located in their states.

In Washington, D.C., the court finally ruled (14 May 1976) that HEW had the power to withhold money on the basis of tolerance levels, but enjoined it from doing so based on its promulgated tolerance levels to the plaintive states because these levels were "framed in an arbitrary and capricious manner." The decision indicates that the court would have sustained the position of the Nixon administration had it shown less zeal to reduce the error rates and, hence, federal welfare expenditures. Secretary Mathews, who had replaced Caspar Weinberger in August 1975, was advised by his general counsel to appeal the decision (interview, December 1977), but the administration, under a new president, mellowed, and Mathews chose to work out more harmonious relations with the states.

Although the states finally received the full federal share of the AFDC expenditures, from 1973 through 1975 the threat of fiscal penalties greatly distressed state officials. A high HEW official reflected on the magnitude of the quality-control change:

> Before this quality-control system, we had federal auditors in the states who would make disallowances for individual cases improperly receiving welfare benefits. The states didn't object to this, for the amounts disallowed on an individual case basis were very small. But with quality control, we went from a small sample and extrapolated the results to the entire universe, and made the disallowances on the total number of errors expected in the universe and not on the number of errors found. All in one fell swoop. The amounts of money involved in the disallowances were staggering (interview, December 1977).

Reorganization. Lay and academic views about the importance of reorganization are diametrically opposed. For citizen groups, which concentrate on reorganization when asked for suggestions to improve government, its importance is paramount. But many academicians conclude that only other factors, not structure, affect policy, and hence play down the importance of reorganization. Both the citizen and academic views are inaccurate–the first for its lack of understanding of other factors, and the second by its preoccupation with them. A reorganization which changes the underlying forces can affect policy. It is a powerful tool in the hands of individuals knowledgeable about organizational dynamics.

The Social and Rehabilitation Agency underwent a great many organizational changes over the course of its 10-year life. In the Nixon administration four types of organizational change produced an increasingly restrictive welfare policy: (1) forcing the atrophy of the Social and Rehabilitation Service, (2) dismantling the Assistance Payments Administration, (3) expanding the non-programmatic staff offices in SRS and contracting the programmatic units, including APA, and (4) regionalization.

Atrophy of SRS. The creation of the Social and Rehabilitation Service in 1967 held out the promise of a focal point in HEW for all social welfare programs. The promise went unfulfilled. Over the years, reorganization ravaged SRS, leaving it an organizational weakling in social-welfare programming. The *coup de grace* came in March 1977, when it was reorganized out of existence (DHEW news release, 8 March 1977).

The heart of the welfare bureaucracy was found in the Assistance Payments Administration, which assumed responsibility for the categorical public-assistance programs, including Aid to Families with Dependent Children, Aid to the Blind (AB), Aid to the Permanently and Totally Disabled (APTD), Old Age Assistance (OAA), as well as services provided to recipients of these programs (except for those handled by the Children's Bureau, such as foster parent care).

After 1969, a series of organizational changes in the Social and Rehabilitation Service downgraded welfare by shrinking SRS, especially its national office. As one programmatic unit was added to SRS, three were removed. The Community Services Administration (CSA), came to SRS in September, 1969.[2] Shortly thereafter, the Children's Bureau shifted from SRS to the new Office of Child Development in the Office of the Secretary of HEW. Removal of the Children's Bureau—even though a stodgy and unimaginative agency (Steiner, 1976, p. 37)—left SRS with less capability to coordinate welfare programs.

On April 1, 1973, an Office of Human Development, headed by an HEW assistant secretary, came into existence and immediately absorbed the Administration on Aging from SRS. The Rehabilitation Services Administration finally fell to the same office on February 2, 1975. Three programmatic agencies remained in SRS: the Assistance Payments Administration, the Medical Services Administration, and the Public Services Administration (formerly Community Services Administration). Employment in programmatic units at the national headquarters of the SRS dropped from 705 in 1969 to 279 in 1975, leaving SRS a hollow shell of its former self (unpublished data, Social and Rehabilitation Service).

Dismantling APA. Another way to reduce old-line bureaucratic influence in welfare policy was to dismantle and bypass the Assistance Payments Administration, which inherited most of the welfare responsibility from the Bureau of Family Services (BFS) in 1967. This began early, almost unwittingly, when the Community Services Administration was created as a unit of SRS in 1969. At that time, the social services function was separated from maintenance assistance and shifted from APA to CSA (HEW *Annual Report*, 1970, p. 92), thereby fragmenting the welfare system.

Ironically, for the cost-conscious, management-oriented Nixon administration, this separation fueled the subsequent explosion in the costs of services. While encouraging states to provide services, the old-line welfare professionals in BFS and later in APA insisted that federal funding be limited to case work which emphasized the interaction of the case worker with the client. The separation of services severed the old-line professional control and coordination of payments and services and allowed the social-services grants program to become distorted into a "gusher of federal funds" for all kinds of activities in states politically able to exploit certain loopholes (Derthick, 1975, pp. 4, 18–19). Tragically for many clients, this failure to stem the costs spurred the management-oriented administration to produce the promised cost reductions in all areas of welfare. Thus, a cost explosion, caused in part by a shift of responsibility away from welfare professionals, prompted the administration to take away even more of their responsibility. The dismantling of APA continued.

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[2] The Community Services Administration in HEW is not to be confused with the better-known Community Services Administration created by the Head Start Economic Opportunity and Community Partnership Act of 1974 (88 Stat 2291) as a successor to the Office of Economic Opportunity. The existence of the two CSA's created untold confusion and HEW responded by changing its CSA to the Public Services Administration (41 Federal Register 1937, 13 January 1976). Many welfare professionals objected unsuccessfully to the new name because of its identification with state public service commissions which perform an entirely different function.

Nixon officials planned to have the Social Security Administration administer the Family Assistance Plan (FAP), clearing the way for the elimination of most of the welfare bureaucracy, an important goal for President Nixon who welcomed the assurances of urban affairs counselor Daniel Patrick Moynihan that FAP would "wipe out" social workers (Burke and Burke, 1974, p. 99). During the period that FAP was under consideration, the APA languished. It had no involvement in the FAP planning, vacant positions went unfilled, and morale was low. While the welfare load increased nationally from under 5 million recipients in 1968 to 11 million in 1972, APA personnel in Washington dropped from 128 to 116. When FAP was finally laid to rest, hopes rose in APA for more staff and resources. Instead, the dismantling was accelerated. By 1975, the number of personnel was down to 89.

The most visible aspect of the dismantling came about in January, 1974, with the federalization of the adult categories (AB, APTD, and OAA) under the Supplemental Security Income (SSI) program. The welfare professionals in SRS lost a large portion of their work load to the insurance professionals in the Social Security Administration, with early estimates of 100 positions to be shifted from APA to SSA (U.S. House Appropriations Committee, FY 1974, Pt. 5, p. 578). When many of the problems predicted for SSA administration of the adult categories materialized (Mullis, 1975), those wanting the welfare program left in SRS felt vindicated.

Less visible, but of greater significance for overall welfare policy, was the early 1973 transfer of quality control from the Assistance Payments Administration to the Office of Management, a unit attached to the SRS administrator (HEW, SRS, Office of Quality Control Management, 1973). This transfer has policy significance because operatives in APA, sensitive to the problem of applicants eligible for welfare but denied access to the rolls, wanted to improve quality-control methods to include examination of negative case actions. With APA not responsible for quality control, demands for review of negative case actions were ignored by appointive officials until December 1976, when, as I have said, they were prodded by a federal judge. As one APA official observed:

> The goal of [SRS administrator] Mr. Dwight is to cut down on the errors picked up by quality control. In so doing, he is ignoring some other matters that are important, but APA has not been allowed the staff to pursue them (interview, March 1975).

Expansion of Nonprogrammatic Staff. Although Harold Seidman (1975, p. 57) notes that the Congress is "grudging in granting to department heads the resources which they believe are required to coordinate and manage effectively the programs for which they are responsible" such as additional staff, the fact is that the federal government staff units have increased faster than line units in recent years (Kaufman, 1976, p. 39). In SRS, staff providing assistance to the administrator proliferated while reorganizations and unfilled vacancies constricted the programmatic units, as the accompanying table shows.

From 1968 to 1974 in SRS, nonprogrammatic staff rocketed from 29.7 percent to 66.6 percent of total staff. (For 1975, the percentage dipped slightly, providing some evidence that, under President Ford, managerial emphasis in welfare had peaked.) In the regional offices, where the staffing figures are harder to

Table 1. SRS Staffing, 1968 through 1975 (July 1)

Year	Programmatic	Nonprogrammatic	Nonprogrammatic as Percent of Total SRS
1968	472	199	29.7
1969	705	406	36.5
1970	687	484	41.3
1971	649	505	43.8
1972	644	581	47.4
1973	482	574	54.4
1974	306	608	66.5
1975	279	480	63.2

Source: Based on unpublished data from the U.S. Department of Health, Education, and Welfare, Social and Rehabilitation Service.

assemble, the SRS policy at this time was also to install more managerial personnel at the expense of line staff, as indicated in 1973 during congressional testimony by the SRS acting administrator:

> It has only been in the last year that we have attempted to put management type people rather than programmatic people in the regional offices. This has shown some good results (U.S. House Appropriations Committee, FY 1974, Pt. 5, p. 578).

At both the national and regional levels, a growing management staff monitored, analyzed, coordinated and otherwise controlled a diminishing SRS program.

As the SRS staff expanded, it absorbed many of the functions which had been performed in the programmatic units, such as public information and affairs, grants management, and most importantly, review and evaluation. As the functions went up to the fifth floor of HEW's Switzer Building in Washington (near the administrator's office), another significant change took place: the commissioners of the programmatic units moved to the fifth floor to be next to the administrator and further removed from the careerists. As one respondent recounted: "It used to be that the commissioner was on the fourth floor, surrounded by his people. After the move, it was hard to get to him" (interview, December 1978).

The increasing management staff in SRS strengthened the Nixon administration's control over policy by reducing the influence of the programmatic units which have a tradition of professionalism. With their employees having backgrounds in social welfare work and associated with the relevant professional organizations, the programmatic units have furthered professional goals and objectives. Although politically appointed, the heads of the SRS units also have been associated with the respective professions, a difficult-to-break tradition. For example, appointing a person with no experience in social welfare to be commissioner of the Assistance Payments Administration would generate considerable political opposition. Appointing the same person to a nonprogrammatic unit would pass unnoticed. Such was the case with Francis DeGeorge, named associate administrator for management in SRS in December, 1972. He came to the welfare programs with a Master of Business Administration degree, a Bachelor of Science degree in accounting, and work experience as a treasurer, vice-president, controller, and cost-accounting manager of several private firms, including divisions of Litton Industries (U.S. House Appropriations Committee, FY 1974, Pt. 5, p. 269). The increase in staff units facilitated this type of appointment to SRS. Lacking ties and commitments to the social welfare profession, the army of management experts installed in these newly created, nonprogrammatic units, in response to administration desires for a more restrictive welfare policy, came to have a profound impact. DeGeorge played a crucial role in the welfare retrenchments during the height of the Nixon campaign to restrict welfare (Derthick, 1975, pp. 96–99).

In the creation of staff units in SRS and the de-emphasis on programmatic units, the Nixon administration selected an effective tool for changing welfare policy. Although the roots of the generalist, centralized control of welfare extend back into the Johnson administration with the creation of the Social and Rehabilitation Service, the Johnson administration neither consolidated control at the SRS level nor reversed the generally expansionist policy ingrained in the operations of the agency and its predecessors since the mid-1930s. Rather, the centralization of control and the reversal of the policy awaited the Nixon administration which adroitly used and further refined the organizational structure conveniently provided by the preceding administration.

Regionalization. The keynote of the Nixon administration's new federalism was "decentralization." In his 1971 State of the Union speech, using a language which became a familiar refrain, President Nixon declaimed:

> The time has now come in America to reverse the flow of power and resources from the States and communities to Washington and start power and resources flowing back from Washington to the States and communities and, more important, to the people, all across America.
>
> The time has come for a new partnership between the Federal Government and the States and localities—a partnership in which we entrust the States and localities with a larger share of the Nation's responsibilities, and in which we share our Federal revenues with them so they can meet those responsibilities (U.S. House of Representatives, 1971).

While Nixon was talking about decentralizing federal authority to the states and communities, he was instead regionalizing it. Proposals developed in the 1960s by the Bureau of the Budget (now the Office of Management and Budget) influenced his actions but not his words. As an agent of the president, the bureau has long sought some sort of presidential presence in the regions. Because Congress, for obvious political reasons, will not allow Bureau of the Budget regional offices, it turned to other means in the mid-1960s when it perceived the need for better field coordination of federal programs following a rapid growth of grants-in-aid. The bureau developed plans for federal regional councils and the requisite common regional headquarters and boundaries for federal departments, as well as a proposal for granting regional directors more authority over programs in their own departments so that they would be in a stronger position to coordinate with other departments. But the Johnson administration limited implementation of these plans to modest experiments with federal councils in four cities (see Derthick, 1974, pp. 160–63).

In 1969, President Nixon accepted bureau proposals for the common regions and headquarters and the federal councils for all regions, but at least in HEW, the proposal for more authority to regional directors was largely ignored. As the management attitude of the Nixon administration came into focus near the end of the first term, however, the administration granted more authority to the regional directors much as the Bureau of the Budget had proposed to President Johnson years earlier. Despite the New Federalism rhetoric which called for decentralization of authority to the states and communities, this was regionalization. No increase occurred in participation by state and local welfare officials. Just as observers have noted the potential in regionalization for greater central control (Kaufman, 1969, p. 8), welfare programs became more subject to the influence of the Nixon administration. By the spring of 1974, HEW Secretary Weinberger openly acknowledged the redefinition of decentralization to mean regionalization—not a flow of authority to the states:

> By "decentralization," we [HEW] mean a delegation of program authority from a program official in Washington, such as the Commissioner of Education, to his counterpart in a region, such as the Regional Commissioner of Education (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 71).

The result of the regionalization of welfare authority was not so much an increase in coordination (as intended by the bureau in the original proposal) as a shift of influence from Washington-based bureaucrats to regional ones, many of whom were recently hired and more receptive to the new welfare policy. In this way, the "decentralization" involved a managerial master-stroke which severed from influence in welfare decision making many Washington bureaucrats who for 35 years had pressed for increased and more progressive welfare programs. Secretary Weinberger unconsciously hinted at this shift of influence when he placated members of Congress who feared that "decentralization" would produce an extra layer of bureaucracy:

> To the contrary, the purpose of decentralization is *to remove a layer.* Right now in most programs . . . the final decision has to be made in Washington. We are talking about putting that authority, final authority, for whatever function it may be—whether audit, technical assistance, or approval of a grant—in the regional office. . . (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 72, emphasis added).

As the secretary suggested, "decentralization" did "remove a layer"—a layer of Washington bureaucrats.

Prior to SRS regionalization, direct lines of control went from the central office units to regional counterparts. In welfare, officials in the regions involved in the administrative reviews of state programs reported to the Assistance Payments Administration. In 1969, even after the SRS regional commissioners were delegated additional program and administrative authority, the commissioners' involvement in SRS operations (as opposed to strictly administrative chores) was generally limited to "those areas of program operations involving two or more SRS programs" (HEW *Annual Report,* 1969, p. 323). In matters involving one program, guidance and control came from the relevant programmatic unit in the central SRS office.

Regionalization upgraded the role of the commissioners. Personnel in the region now reported directly to the commissioners, who deliberately severed their relationships with counterparts in the national headquarters. Regionalization changed the role of the commissioners from field representatives attempting to conciliate disputes among warring units to agents of the SRS administrator managing all SRS programs. With the administrator and regional commissioners as political appointees, the potential for presidential control of the welfare system increased.

To buttress the administrator's position further, the Nixon administration upgraded the Office of Field Operations (OFO), an SRS unit assigned staff functions but inevitably assuming line control. The administration required that all SRS communications between the regional and central offices be channeled through OFO.

Pressures on central office staff were sufficiently onerous that virtually all written communication and an overwhelming proportion of telephone communications were submitted to this prior clearance.[3] As a result, central office staff lost contact with the field, became less familiar with regional personnel, and were left out of the mainstream of welfare policy.

In the regions, a growing contempt for the Washington programmatic units seemed to have official sanction. One regional official said a report or recommendation goes directly to the SRS administrator: "It would get lost if we sent it to APA; we feed it right to SRS" (interview, June 1975). When asked about the value of APA headquarters staff, for assistance payments work in the region, a regional respondent reported that they are there in Washington "to help us to interpret the federal 'regs.' " But the official added, "We must make most of our decisions [about state plan compliance with federal rules and regulations] on our own; if it is not down in writing, then they are not much help" (interview, June 1975).

Although official spokespersons, for the record, dismissed variations in decisions among the regions following regionalization (U.S. House Appropriations Committee, FY 1975, Pt. 2, p. 72) despite contrary evidence (Derthick, 1975, p. 40), operatives voiced great concern. One administrator reported:

> The ten regional offices don't make consistent decisions. We could have told them, when they were shifting responsibilities to the regional offices, that they would end up with inconsistent decisions (interview, March 1975).

These officials asserted the need, as one put it, for a "systematic mechanism for reviewing regional operations" (interview, March 1975). But the logical agency, the APA, lacked the authority and staff.

During the FY 1975 budget hearings, Congressman Flood (D-Pa.) asked SRS Administrator Dwight if he planned to assign all 204 newly requested positions to the regional offices. Dwight's response evinced his lack of sympathy for the complaints from headquarters about staff shortages:

> Yes. My view . . . is . . . the regions are understaffed and the headquarters may have recently been a little overstaffed. But I think that is largely a question of the past. By not filling vacancies here in Washington, we have pretty well eliminated whatever overstaffing there may be in headquarters (U.S. House Appropriations Committee, FY 1975, Pt. 6, p. 282).

The failure to strengthen the Washington office of the APA effectively deprived it of important functions until its abolition in 1977.

The ostensible decentralization employed in the Social and Rehabilitation Service has been an effective management tool to obtain policy results. Policy objectives of high-level SRS personnel clearly differed from those of the long-term career officials at lower SRS levels, including the operatives in APA. As one official close to the Secretary of HEW observed, "At the highest levels of SRS, an oppressive attitude about welfare is pervasive. Down lower in SRS there is not this oppressive, punitive attitude" (interview, March 1975). Regionalization silenced one of the significant voices of the federal-state welfare bureaucracy.

The Policy Consequences

The overarching consequence for the welfare system of the Nixon administration was an increasingly restrictive welfare policy in the AFDC program. Management tools used by that administration had the direct effect of impeding access to the welfare rolls and the indirect effect of producing a deterioration in the federal-state relationships in welfare policy. While the former was the more visible, the latter may prove more insidious over the long run.

Compared to the states, the federal welfare establishment had long been an important force for inclusion of eligible persons on the welfare rolls. With the help of federally funded antipoverty programs, the greatest gains came in the late 1960s and into the early years of the Nixon administration as the normally cautious state and local welfare systems acquiesced to pressures to enroll clients previously discouraged (Randall, 1976). The quality-control system and other Nixon administration measures were intended to stop the rapid expansion of the welfare rolls. They had an undeniable impact. After 1972, as Figure 1 shows, the AFDC rolls leveled off at about 11.5 million recipients, in spite of a poorly functioning economy which increased the pool of those eligible for welfare. Obviously, welfare is not denied to the vast numbers of those eligible, as was true before the early 1960s. But, when eligibility criteria of each state were applied to the 1970 data files of the Current Population Survey of the U.S. Census Bureau, 22 percent of those eligible for AFDC were not receiving welfare; in the southern states, the exclusion

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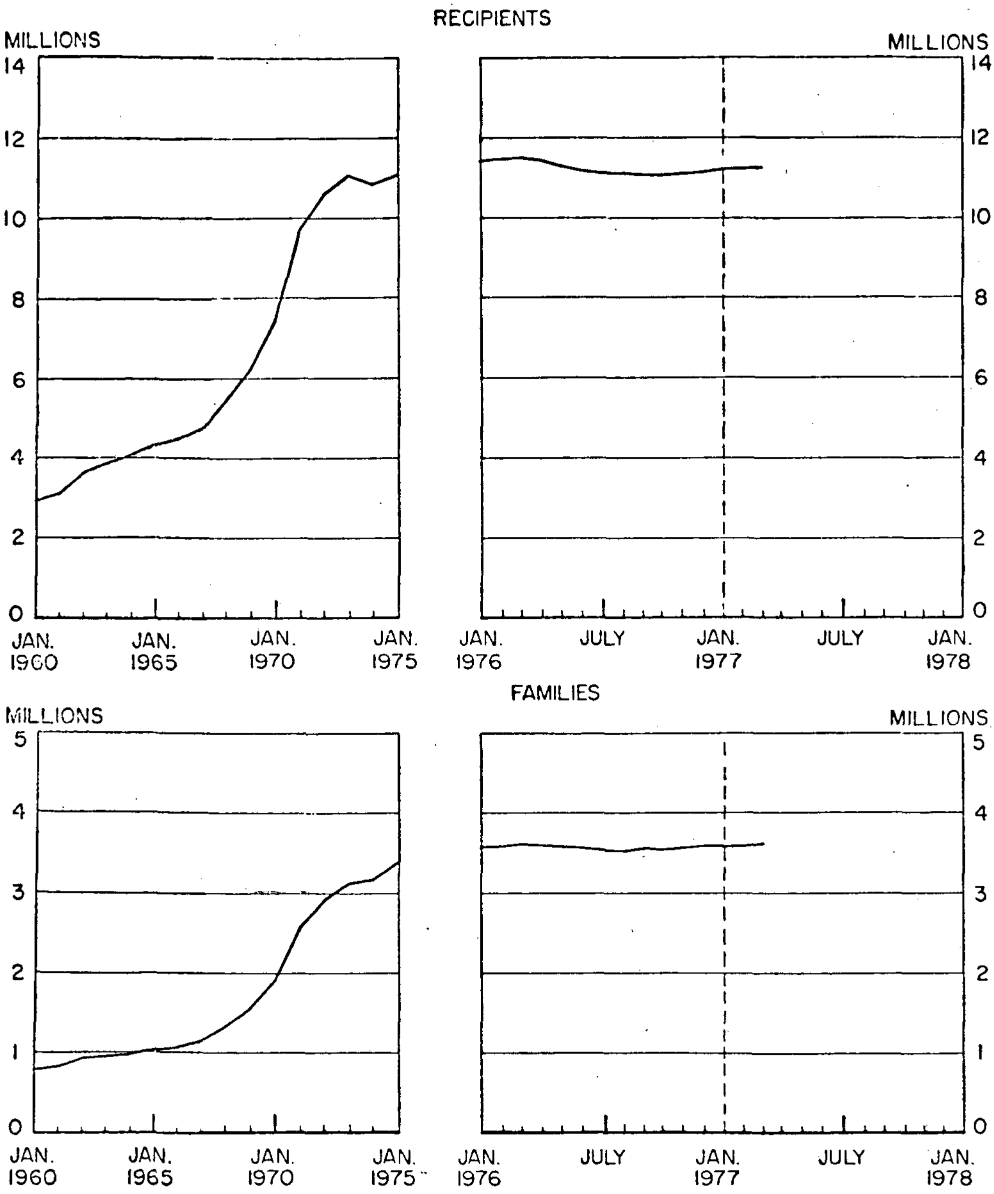
[3] Survey data confirming my argument that there was little communication between regional and central SRS staff can be gleaned from Jun's (1976) study of the SRS Operational Planning System (OPS, a pseudo-Management by Objectives system). His regional office respondents "said the central office staffs did not provide any feedback (or even call them)" about OPS reports. Jun appears surprised to learn that regional officers "in many cases did not reveal their own shortcomings to the central office" (1976, p. 50). Given the administrative effort to eliminate the influence of the welfare professionals in the central office, close observers knew of the paucity of communication between regional and central office counterparts.



Source: U.S. Department of Health, Education, and Welfare, *Public Assistance Statistics* (October 1976 and March 1977).

Figure 1. Aid to Families with Dependent Children

rate rose to 34 percent (Boland, 1974, pp. 152–53). Considering the significant increase in the number of unemployed and the relatively stable welfare load since 1970, it would appear that participation rates decreased during the Nixon administration campaign to restrict welfare. Substantial numbers of welfare eligibles remain unenrolled who can legally claim benefits—a point of no contest to case workers who make eligibility determinations; they will confess to turning away people who probably are eligible.

When quality control shifted from the welfare professionals to the office of the SRS administrator, the means which states used to reduce their error rates became unimportant to the relevant federal officials, as long as the error rates diminished. An SRS official involved in quality-control development reflected the prevailing ideology of the Nixon SRS: "We are more interested in results than in how the states reduce the error rates" (interview, March 1975). This ideology stimulated changes injurious to welfare recipients and inconsistent with the traditional federal welfare posture. One official volunteered: "One state effectively cut down on error rates concerning diabetic diet allowances. They no longer allow special diabetic diet allowances" (interview, March 1975). Similarly, another state defined away the tubercular diet problem.

Using a methodology distrusted in many states and questioned by bureaucrats who used it (interviews, March 1975), Nixon appointees nevertheless claimed precise savings from the quality-control system. SRS Acting Administrator Don Wortman estimated "a direct saving since 1973 [to spring, 1976] of close to $936 million as a result of the quality control program," and that quality-control penalties against 43 states in 1977 would save $220 million (U.S. House Appropriations Committee, FY 1977, Pt. 6, pp. 56, 31), or just under 2 percent of the federal AFDC expenditures of $11.5 billion. An HEW press release later that year (16 December 1976) reported that 490,000 recipients or 175,000 families were "removed" from the welfare rolls because of the quality-control campaign.

Whatever the significance of the quality-control savings, the emphasis clearly displaced other actions. Planning and development in welfare within the Social and Rehabilitation Service count among the casualties. Although former SRS Administrator Dwight once asserted that "our strategy is to reshape the central SRS office into a true national policy and planning agency" (U.S. House Appropriations Committee, FY 1975, Pt. 6, p. 73), few in SRS and no one in APA could point to any significant national policy development and planning, aside from quality control, which had consumed much time in SRS units. Two years later, after pressing the SRS acting director to admit to little SRS involvement in the development of welfare reform proposals then under consideration, Congressman Robert Michel (R-Ill.) questioned the advisability of ignoring experts: "I don't know how you can ever just out of thin air dream up a reform proposal without going to those of your people who work directly on a day-to-day basis with the thing" (U.S. House Appropriations Committee, FY 1977, Pt. 6, pp. 37–38).

Another consequence of the Nixon administration welfare policy was the end of federal encouragement of the states to adopt AFDC-U, the program supplement to AFDC which allows families headed by unemployed fathers to be eligible for welfare. The putative concern of the Nixon administration to keep families together, which was one of the driving forces behind FAP (Moynihan, 1973), could have manifested itself after the demise of FAP in a campaign to extend AFDC-U to the 26 states without it. That, however, would have increased welfare rolls. Consequently the welfare system continued in most states to foster family breakup by requiring that there be no father in the home.

Over the long run, the deterioration of the relationship between federal and state welfare officials may prove to be the more debilitating blow to good administration. From the beginnings of the joint federal-state welfare system through the 1960s, "partnership" fairly characterized the relationship between federal and state officials, with federal officials pressuring for professionalization and expansion of the state programs. For 35 years, the federal welfare establishment provided guidance and encouragement to improve methods of welfare delivery, to design better organizational structures, to upgrade welfare personnel, and to enroll eligible people.

Derthick cites the federal actions which led to changes in Massachusetts. Federal pressure was generally subtle, as she observes:

> Federal influence, in summary, operates mainly through the agency that receives grants, and with the agency's self-serving cooperation. It operates by enhancing the role that the agency plays in the state political system and by shaping the agency's values, goals, interests and actions. It operates primarily on the structures and processes of policymaking and administra-

> tion rather than directly on the substance of policy. This may be a critical limitation, but if the influence on structures and processes is extensive and enduring enough, the result must be to influence policy outcomes as well–all policy outcomes, not just those in which the federal government is actively interested (Derthick, 1970, p. 207).

In one specific example, Derthick (1970, pp. 75–97) shows how Massachusetts gradually shifted to increased state control of local operations until finally, in 1967, the system was formally changed to a state-administered one, largely because of indirect federal pressure. Of course, there has always been official silence on statewide administration at the federal level, but in the 1970s pressure was no longer put on states to move in that direction. According to one well-placed SRS official:

> You will not find an official SRS position on state-administered versus state-supervised systems. Some of us have our private opinions, but there is no official view. I don't think there is any pressure at all on the states to change to a state-administered system (interview, March 1975).

Recent research supports the widespread impression that state-administered systems are more professional and expansionist (Randall, 1976).

Governors, generally uninterested in the details of welfare (e.g., Weinberg, 1977, pp. 140–42), were united in opposition to many Nixon administration welfare actions. For example, Democratic and Republican governors joined in an appeal to the Congress to write the social services law in such detail that the Nixon administration would be unable to deny funding for social services carried out by the state agencies. States were discovering, after billing the federal government for its share of a given social service, that the Department of Health, Education and Welfare no longer considered it as such and thereby denied funding. In his capacity as chair of the Governors' Conference, Governor Daniel J. Evans (R-Wash.) testified that the states no longer trusted HEW and wanted specific definitions of the social services written into the law–rather than leaving the issue to the regulations written by HEW, even though he thought that under other circumstances this "would be unnecessary and undesirable" (U.S. Senate, 1973, p. 12).

Finally, the recent emphasis in federal welfare policy has greatly reduced the once-prevalent technical assistance to state and local welfare operations. After 1973, regional officials explained that technical assistance was one of their major responsibilities. As an example of technical assistance, one regional assistance payments official discoursed on "the help they can provide states in preparing their corrective-action plans to improve the quality-control results." When asked for a specific example of help on a state corrective plan, she said: "Well, most states don't ask for much help." Of the state for which she was responsible, she added: "They haven't asked for help, either" (interview, June 1975).

The view from the state level does not change the impression of a breakdown in federal technical assistance. Regarding help on corrective-action plans, a high official in Ohio welfare said: "We get no technical assistance from the regional office. On the corrective-action plan, we go to the other states for help and to see what they are doing" (interview, August 1975).

Concerning potential help from the regional office, a state official with lengthy experience in two different regions observed that until 1970 or 1971

> there were knowledgeable people in the regional office. Now they seem to have all new people who don't know the law or the regulations, or very much about welfare. We see a lot more use of management consultant firms (interview, August 1975).

Isolating the welfare professionals and assigning additional responsibilities to the new management-oriented employees assured a federal-state relationship distasteful to many. In pleading for a return to the old relationship, an old-line Washington bureaucrat said:

> You have to understand that public-assistance program has traditionally been a joint program. It has operated as a joint program. The feds have not in the past been sneaking around spying on the states. We have worked together (interview, March 1975).

A state official succinctly characterized the change in the federal-state relationship: "From help and assistance to an adversary role" (interview, August 1975). Some SRS policy makers publicly agreed with this assessment, including SRS Acting Deputy Commissioner Mike Suzuki (1975, p. 12) who summed up social services during this period thus: "Rather than two levels of government brought together in a common enterprise, the federal-state relationship became that of adversaries." Behind this deterioration of federal-state relations is the control exercised through the management techniques of the Nixon administration. States were under the gun to make their programs more restrictive; the management-oriented officials in Washington and the regions held the gun.

Summary and Conclusions

Many students of the presidency stress the obstacles to presidential control of the bureaucracy, basing their findings on the recollections by presidents, their aides and bureaucrats of presidential-bureaucratic conflict. This research approach runs the risk of letting the respondents write the conclusions. If one shifts from questions about presidential-bureaucratic relations in the abstract to analyses of the management techniques used by presidential administrations in specific policy areas, one's perspective is likely to change. Presidents do enjoy policy successes in the face of bureaucratic opposition, as did the Nixon administration in the area of welfare. When presidents fail to influence policies, it is not usually for lack of administrative tools. Rather, the explanation may be a lack of time, lack of political or personal interest, or lack of understanding of the bureaucracy and management tools at hand. Perhaps because recent presidents generally have concluded that they can best secure a prominent place in history with an impressive legislative box score, matters of management have too often been shunned (Seidman, 1975, pp. 71–73). Moreover, most incoming presidents are inexperienced in managing government programs and must learn on the job.

During the first two years of his administration, President Nixon emphasized a legislative over a managerial strategy to achieve policy goals. Faced with few legislative achievements, and concluding that "operations is policy" (Nathan, 1975, p. 62), he later switched to a managerial strategy to change policy, characterized by Nathan (1975, p. 8) as the "Administrative Presidency." A new set of management-oriented leaders replaced another set which had been implementing a traditional federal welfare policy distasteful to the administration. These new leaders wasted little time in employing management techniques to turn welfare policy in a restrictive, sometimes punitive, direction. They scuttled the old-line units and turned to more responsive ones, often newly created in the national and regional offices. They forced the atrophy of the Social and Rehabilitation Service and they dismantled the Assistance Payments Administration. Management units with no programmatic experience were given programmatic responsibilities. They developed a monitoring system which forced case workers to become more restrictive in determining eligibility and payment levels. They refined that system to exclude from consideration the concerns of old-line professionals. They reduced the technical assistance to the states. They turned from helping hapless individuals to constricting the system.

Before generalizing from this case study, we must note factors peculiar to the welfare system which enhanced presidential control. First, the AFDC program does not enjoy widespread support among the general population. Efforts to constrict the system are aided by this low legitimacy. Second, the beneficiaries of the program are poor, not well educated, and lacking in political efficacy. They are not effective defenders of their interests, a factor which also buttressed the administration's position. A third factor which might appear to have aided the Nixon administration was its desire for a retrenchment rather than an expansion of the program. However, while retrenchment might normally be easier to achieve through presidential management efforts, a mandated program like welfare presents a special case. Because the open-ended appropriations of a mandated program automatically provide whatever amount of money is necessary, retrenchment and expansion would be about equally difficult.

On balance, the conditions peculiar to welfare should strengthen the president's managerial hand in this program as compared to others. But there remains the possibility that presidential management efforts can be handled more adroitly, offsetting in some degree the management difficulties characterizing other program areas. For example, welfare changes sought by the Nixon administration were limited by its excessive zeal in its financial dealings with the states and by Nixon's failure to serve an entire second term.

The experience of the Nixon administration with the welfare bureaucracy suggests how a president can short-circuit the legislature and put a personal policy stamp on the programs administered by the bureaucracy free from the glare of publicity, congressional control and the awareness of the general public. Yet the Nixon administration enjoyed greater success over the federal welfare bureaucracy and its policy than it did over welfare outcomes—the policy at the operating level experienced by those eligible for or claiming welfare. In obvious and not-so-obvious ways, federalism complicates efforts to control welfare policy. Because state welfare systems have always maintained considerable autonomy, control over the federal welfare bureaucracy does not translate into a similar degree of control over the state systems. Rather, control over, or neutralization of, the former must precede control over the latter. In this sense, Nixon's mid-term resignation robbed his administration of the time needed to develop more extensive control over the state welfare systems. In fact, almost immediately after his resignation, the pressures on both federal welfare officials and the states abated. In addition, many state welfare systems were generally closer in ideology to Nixon than they were to the federal bureaucrats. In part, then, administration gains over the federal bureaucracy simply relaxed pressure on many states to do what neither they nor the Nixon administration wanted done.

But an additional and more subtle factor involving federalism is necessary to explain why changes in welfare outcomes did not match the changes in the federal welfare policy. Although the deterioration in federal-state relations worked to the advantage of the Nixon administration policy preferences, the federal system has recently undergone other changes which inhibit presidential control of domestic policy. The states themselves—through the actions of governors and other top officials concerned with overall state budgets—are becoming a different kind of element in the federal system as their fiscal solvency depends increasingly on federal dollars. Where the financial stakes are great, states have been transformed into a separate clientele of the federal government, so that they act more as powerful claimants on the federal treasury (much like many other powerful interest groups) than as partners of the federal government administering a joint federal-state program. Recall two Nixon administration ventures to reduce the flow of federal welfare funds to the states. Successful opposition came from the states; that the interests of the states, of the federal welfare bureaucracy and of welfare recipients merged was fortuitous for the latter two. First, Democratic and Republican governors descended on Congress in 1973, forcing the Nixon administration to retreat from its efforts to redefine many social services out of existence (and thereby reduce the flow of federal dollars to the states). Second, the fiscal penalties that the Nixon administration planned to impose for high error rates uncovered by the quality-control system unified the states in successful opposition through the courts.

Where the financial stakes are not so great, the states' political involvement in policy and program administration will recede, more typical federal-state administrative patterns will obtain, and governors will be relatively indifferent to presidential efforts to control domestic policy. Where financial stakes are large, however, the states become involved as a powerful class of actors both more removed from presidential control than the federal bureaucrats and with greater access to the Congress. Moreover, members of Congress have become more receptive to the pleas of the states (and other constituents) in recent years as "a lesser proportion of congressional effort is now going into programmatic activities and a greater proportion into pork-barrel and casework activities" (Fiorina, 1977, p. 46).

President Nixon's domination of the federal welfare bureaucracy and the considerable changes he wrought in welfare policy and outcomes raise important questions about the limits to presidential control of policy. If there are limits, they are ambiguous and variable. Laws usually fail to delineate them clearly. They are reduced when a president seeks retrenchment of a program beneficial to a politically weak clientele, or perhaps when seeking expansion of a program benefiting a powerful clientele. In this study, limits seemed to emerge as presidential actions drew the states into the fray. In American politics, important issues are rarely settled permanently. Opposition from other power centers like the states not only prevents a presidential initiative from being implemented in the manner intended, but also means that changes made by a presidential administration will never be irreversible. Before taking too much comfort in this kind of conclusion, however, consider that the limits noted in this case study could have been forced back even further and welfare outcomes changed more dramatically had the Nixon administration pursued a more moderate strategy with the states and had it survived another two years. Hence, the greater the president's managerial astuteness and determination to control, the further removed are the limits. Clearly, the power over the bureaucracy and control over policy is subject to abuse, which makes the study and surveillance of presidential management efforts so important.

Clearly, management techniques should be used in any paradigm of presidential policy development and implementation. Obviously the specific techniques found important in this case study of welfare will not necessarily be important in other areas. However, analyses of other policy areas in which the national government has a significant involvement should begin with the expectation that there are great resources available to the administratively astute president disposed to use them.

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Generational Replacement and the Growth of Incumbent Reelection Margins in the U.S. House*

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Virtually all congressional scholars investigating the rise of incumbent safety in the U.S. House have assumed that the responsible cause, regardless of its specific nature, is one which has affected incumbents generally. The sole exception is Fiorina, who speculates that increased overall safety results from recent freshmen's greater electoral strength. The analysis performed here confirms this generational replacement hypothesis. Much greater vote gains have resulted from the freshman term of incumbency since 1966–68, while no pro-incumbent trend for veterans has surfaced.

> **Incumbent,** n. A person of the liveliest interest to the outcumbents.
>
> —Ambrose Bierce, *The Devil's Dictionary*

Students of congressional elections, like the "outcumbents" who must wage ever more Sisyphean campaigns, have demonstrated the "liveliest interest" in the growing electoral value of incumbency. Alternative explanations for the phenomenon have focused on redistricting following the 1964 *Wesberry* v. *Sanders* ruling (Tufte, 1973, pp. 551–53; 1974), expanded use of perquisites like the franking privilege (Mayhew, 1974, p. 310; Cover, 1977, pp. 536–39; Cover and Mayhew, 1977, pp. 65–66), increased case-work services (Fiorina, 1977a; 1977b, pp. 41–49), direction of additional federal funds into the district (Fiorina, 1977a; 1977b, pp. 41–49; Mayhew, 1974, p. 311), more skillful "position-taking" (Mayhew, 1974, p. 311), and the decline of partisan voting cues (Ferejohn, 1977, pp. 172–75; Erikson, 1972, p. 1240; Burnham, 1974; Mayhew, 1974, p. 311; Cover and Mayhew, 1977, pp. 66–68; and Nelson, 1978–79, pp. 665–78).

The all-but-unanimous assumption of these scholars, however, is that whatever the cause of greater reelection margins, it has had a general impact upon incumbents. Fiorina alone speculates, without attempting an empirical test, that the observed increase in safety actually results from the greater electoral strength of freshmen entering Congress since the mid-60s (1977b, pp. 54–55). This study will confirm Fiorina's generational replacement hypothesis by demonstrating that House cohorts elected since 1966 have achieved more electoral value from their incumbencies than those they replaced, while members of the pre-1966 generation have not benefited from any general pro-incumbent swing.

Why Has Incumbent Safety Risen?

Results for the 13 congressional elections from 1952–76 will be investigated here. Incumbents running in multi-member districts, as well as those elected on an at-large statewide basis, are excluded from the calculations. On the other hand, the analysis will include districts subjected to regular decennial redistricting or to boundary adjustments necessitated by the 1964 *Wesberry* v. *Sanders* decision. Ample evidence has been presented by Ferejohn (1977, pp. 167–68), Cover (1977, pp. 528–31), Bullock (1975), Mayhew (1974, pp. 303–06), and Erikson (1972, p. 1241) demonstrating that the electoral effects of both kinds of redistricting have been insignificant.

Previous studies have chosen to quantify the electoral safety of incumbents in terms of the percentage of reelection bids falling within marginal vote categories. Mayhew (1974, pp. 303–04), for example, finds that regardless of whether a 55 or 60 percent cut-off point is established, the number of marginal incumbents has fallen by about half from 1956 to 1972. A somewhat less dramatic decline in marginality has been discovered by Tufte (1974), who analyzes incumbent safety by computing the percentage receiving less than 53 percent of the vote. While the marginality measure does indicate the dwindling number of districts with incumbents most vulnerable to defeat, it still ignores possible shifts among districts regularly outside the marginal range. It cannot reveal whether the movement of marginal districts into safer categories has, for instance, been

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counterbalanced by a reduction in the number of seats which formerly were won by extremely lopsided majorities. Furthermore, if many incumbent seats normally cluster around the cut-off point employed, modest swings of the national vote can produce significant fluctuations in the percentages of incumbents judged to be marginal.[1] The measure has limited utility, therefore, as a summary indicator of incumbent safety.

Electoral safety here will be quantified more comprehensively as the incumbent's margin relative to the mean vote received by all opposed congressional candidates of his or her party. This correction is needed to control for changes in safety which can be produced by national electoral swings when there is substantial disparity between the numbers of incumbent Republicans and Democrats. Consider, for example, the effect of a 5 percent pro-Republican inter-election swing upon the margins of 160 Democrats and 90 Republicans seeking reelection in both elections. In the absence of any real increase in the electoral advantage of incumbency, the average margin nonetheless would appear to change by

$$\frac{160(-5\%) + 90(5\%)}{250},$$

or −1.4 percent. We thus remove any biasing effects of national swings by subtracting from the opposed incumbents' percentage of the two-party vote the mean share of the contested two-party vote received by all incumbent and non-incumbent congressional candidates of their party that year.[2] (All election data are taken from the biennial editions of *American Votes*, edited by Richard M. Scammon.)

The last column in Table 1 indicates the average magnitudes of these corrected reelection margins from 1952–76. Clearly, the data fall into two distinct periods: 1952–64, and 1966–76. The incumbent margins for the latter period are higher than any of the margins for the years 1952 to 1964. From 1952–64, the average incumbent safety level is .114, and there is no discernible trend; the equation for safety (Y) regressed against time (t) has zero slope ($Y = .113 + .000t$, $r = .095$). Margins from 1966–76, averaging .144, increase slightly over time ($Y = .137 + .002t$, $r = .412$). The two most recent averages for 1974 and 1976, however, represent declines from the peak of incumbent safety attained in 1972.

Except for Fiorina, researchers who previously have uncovered increases in safety using their own indicators have attributed this development to a general pro-incumbent trend. Underlying their belief is the assumption that any changes in the overall electoral value of incumbency will be manifested approximately to the same degree across seniority classes. This is evident in Cover's recent use (1977, p. 527) of the so-called "sophomore surge" measure—the average vote gain of freshmen between their initial victories and first reelection tries—to quantify the electoral advantage enjoyed by incumbents; increases in "sophomore surge" values are assumed to parallel similar safety improvements among veteran members.

The only data actually bearing on this assumption, however, have been provided by Erikson in an analysis of congressional elections from 1960–70. Separate calculations of the "sophomore surge" are not presented; instead, Erikson chooses to quantify the incumbency advantage with a hybrid measure based upon a weighted average of the "sophomore surge" and the loss experienced by a party when its incumbent retires. Still, since freshman reelection bids in most years outnumber incumbent retirements, we would expect the values of Erikson's measure to approximate those of the "sophomore surge." Discovering that a 1964–66 increase from about 2 to 5 percent in the measure is matched by a similar 3-percent rise in the margins of veteran incumbents, Erikson states that "the electoral advantage from being an incumbent simply increased suddenly with the 1966 election" (1972, p. 1240).

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[1] Similarly, Burnham (1975, p. 424) shows that substantial inter-election swings, like the aggregate 6-percent national shift to the Democrats from 1972–74, can produce very large changes in the percentage of marginal incumbents. Because of the Republican decline over these two years, the number of all northern and western incumbents receiving less than 60 percent of the vote jumped by fully 11 percent.

[2] In order to compute national interelection partisan swings, Tufte (1973), Mayhew (1974a), Cover (1977), and Cover and Mayhew (1977) measure a party's national strength as the percentage of the aggregate vote received by all its congressional candidates, regardless of whether they run unopposed. This would seem to overstate somewhat the strength of the Democratic party in its most successful years, since the number of its incumbents running unopposed in these elections is higher than normal. Hence, we rely here, as well as in later calculations of national inter-election swings, upon the mean party vote in contested districts only.

However, any conclusion drawn from this finding that the electoral movements of freshmen and veterans more generally are driven by a single incumbency advantage factor must be qualified by the fact that Erikson's analysis excludes the substantial number of districts which either are redistricted or located within southern and border states. In the 1964–66 analysis, for example, he is limited to including only about half (122) the total number of veteran incumbents (245) who actually faced major party challengers in both elections. But even if attention is confined to Erikson's limited data set, there is reason to wonder whether the parallel between the 1964–66 changes of his measure and the reelection margins of veterans may itself be only a temporary aberration. His own findings for the 1966–68 and 1968–70 election pairs show that while northern veterans continue to pick up an additional 1.6 percent over these four years, the measure still remains at its 1966 value of only 5 percent (Erikson, 1972, p. 1240).

The assumption by researchers that the greater safety of seats since the mid–60s results from a general increase over all seniority classes, probably spawned in large part by Erikson's early work, thus remains to be confirmed. In contrast to this historical change hypothesis, Fiorina's generational replacement explanation suggests that members of Congress elected since the mid–60s simply may have derived more electoral mileage from their incumbencies than more senior members. If generational replacement is, in fact, responsible for the rise in safety, the average margin of Congress members in the aggregate should increase when the first class of the new generation makes its initial reelection bid, and should then continue to grow as successive cohorts replace members of the old generation in future elections. We shall test these two hypotheses across the complete cross-section of congressional districts by examining the inter-election movements of freshmen and veteran incumbents facing major party opponents in both elections, relative to the average biennial swing in all U.S. districts which have major party competition both times.[3]

Table 1. Electoral Safety of Incumbents with Major Party Opposition

Year	Number of Incumbents Running	National Democratic Mean	Average Incumbent Margin	Average Incumbent Margin Relative to National Party Mean
1952	286	.480	.622	.119
1954	313	.511	.606	.109
1956	325	.499	.608	.108
1958	285	.550	.615	.121
1960	313	.534	.615	.109
1962	315	.520	.621	.120
1964	340	.570	.619	.114
1966	340	.502	.629	.128
1968	347	.508	.643	.143
1970	324	.531	.655	.154
1972	319	.518	.656	.154
1974	316	.572	.642	.143
1976	330	.563	.659	.143
Average Incumbent Safety, 1952–64			.615	.114
Average Incumbent Safety, 1966–76			.647	.144

Source: Based on data from Richard M. Scammon (1956–1977), *America Votes*, Vols. 1–12, New York: Macmillan; Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: All margins are calculated as percentages of the 2-party vote. National party means are the mean party percentages of the 2-party vote in all contested open and incumbent-held seats. Average incumbent safety for 1960–64 and 1966–76 has been computed by weighting yearly averages by number of incumbents running.

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[3] As above, all electoral percentages are computed on the basis of the two-party vote. Members who initially win their seats in special elections held within one year of the next general election are included with those first winning their seats in that general election when freshman inter-election swings are calculated.

These data, presented in Table 2, reveal that freshmen from 1952–54 to 1962–64 gain an average .015 of the vote between their initial victory and first reelection. Veteran margins over these years do not change. Between 1964 and 1966, there is a sudden rise in the electoral strength of all incumbents; the average margins of both seniority groups increase substantially beyond the mean values recorded previously.

The veteran surge, however, is dissipated over the next five elections. After a .007 drop from 1966–68 and a .013 increase from 1968–70, veteran margins then decline in each of the three remaining election pairs. By 1976, in fact, the net .025 electoral loss of all non-freshmen since 1966–68 has more than offset the abrupt .021 gain of 1964–66. In contrast, freshman inter-election gains rise spectacularly to .072 from 1966–68 and then continue through 1974–76 at levels considerably higher than those attained by pre-1966 freshman cohorts.

Across the 1952–54 to 1964–66 period, the assumption by previous researchers, that a single incumbency advantage factor similarly affects all seniority classes, seems well founded. Shifts in the electoral payoff of the first term tend to covary with shifts in the margins of more senior members; the Pearson correlation between changes in successive "sophomore surge" values and veteran inter-election vote changes is a moderately strong .456. From 1966–68 to 1974–76, however, the corresponding coefficient is only .075, indicating almost complete independence between the electoral movements of freshmen and veterans.

The sizeable gains of first-termers from 1966–68 to 1974–76, averaging .066, are thus produced by factors which are unique to cohorts elected since 1966, rather than the result of a general electoral increase across all seniority classes. While a correspondence between the vote changes of freshmen and veteran incumbents does exist until 1964–66, it disappears thereafter as freshmen continue to achieve enhanced electoral gains which are independent of veteran performance. The general incumbent surge of 1964–66, emphasized by Erikson in his analysis of northern elections, is revealed here to have only transitory effects nationally which are cancelled by the veteran declines of succeeding years. Were it not for the ability of the new-generation freshmen to reap greater electoral rewards from their first term, the increases in incumbent safety demonstrated in Table 1 could not have occurred.

Generational Replacement: What Electoral Difference Does It Make?

While it is apparent that generational re-

Table 2. Inter-Election Vote Shifts of Freshmen and Veteran Incumbents, Relative to National Party Swing

	Freshmen	Veterans
1952–54	.014 (67)	–.004 (225)
1954–56	.019 (45)	–.007 (254)
1956–58	.015 (42)	.019 (237)
1958–60	.000 (76)	–.012 (209)
1960–62	.025 (56)	.006 (227)
1962–64	.023 (54)	–.005 (256)
1964–66	.035 (76)	.021 (245)
1966–68	.072 (66)	–.007 (253)
1968–70	.064 (33)	.013 (273)
1970–72	.076 (50)	–.006 (246)
1972–74	.058 (60)	–.023 (230)
1974–76	.062 (92)	–.002 (212)
Average 1952–54 to 1962–64	.015 (340)	.000 (1408)
Average 1964–66 to 1974–76	.060 (377)	.000 (1459)

Source: Based on data from Richard M. Scammon (1956–1977), *America Votes*, Vols. 1–12, New York: Macmillan; Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: National party swings are calculated in terms of mean party shift in all open and incumbent-held seats with 2-party competition in both years of election pair. Numbers of incumbents with major party opposition in both years of each election pair are in parentheses. Averages for 1952–54 to 1962–64 and 1964–66 to 1974–76 have been computed by weighting biennial shifts by number of incumbents running.

placement has been responsible for the greater safety of incumbent seats, we have yet to specify its exact impact in each election year. What levels of safety would have occurred in 1968 and afterwards, were it not for the greater electoral strength of new-generation cohorts? The influence of each new freshman class in raising overall incumbent margins will depend not only on the magnitude of its first-term gains, of course, but also on the number of its members. Likewise, the overall effect of inter-election movements by veteran incumbents opposed in both elections of the pair will depend upon the numbers involved. Both of these influences will be measured below, as well as the impact of a third, less obvious factor—biennial shifts of incumbents between unopposed and opposed election status. A Congress member running unopposed is excluded from the calculation of incumbent safety we have performed above for that year, but is included in the computation for a second election if major party opposition surfaces at that time. Since the two-party percentage of the vote attained by such a member in the second election is likely to be very high, the incumbent average will be raised. The technique used here to decompose biennial changes in incumbent safety into the three components is explained in the appendix.

Table 3 presents the values of these components for each election pair.[4] Over the 1952–54 to 1962–64 period of incumbent margin stability, the mean electoral impact of each influence is fairly negligible. From

Table 3. Decomposition of Inter-Election Changes in Incumbent Safety

	Veteran Vote Shift Impact	Generational Replacement Impact	Unopposed-Opposed Electoral Transition Impact	Net Change Due to All Impacts	Incumbent Safety in Latter Year of Each Electoral Pair from 1966–68 to 1974–76 if Generational Replacement Impact Remained at 1952–54 to 1962–64 Mean
1952–54	–.004	–.001	–.006	–.010	–
1954–56	–.006	.003	.002	–.001	–
1956–58	.018	.004	–.010	.013	–
1958–60	–.010	–.010	.008	–.013	–
1960–62	.006	.009	–.005	.011	–
1962–64	–.005	.003	–.002	–.005	–
1964–66	.018	–.004	–.001	.014	–
1966–68	–.006	.023	–.001	.015	.122 (.143)*
1968–70	.012	.000	–.003	.011	.135 (.154)
1970–72	–.005	.007	–.002	.000	.129 (.154)
1972–74	–.023	.011	–.001	–.011	.108 (.143)
1974–76	–.001	.000	.001	–.001	.109 (.143)
Average 1952–54 to 1962–64	.000	.001	–.002	–.001	–
Average 1964–66 to 1974–76	–.001	.006	–.001	.005	.120 (.147)

Source: Based on data from Richard M. Scammon (1956–77), *America Votes,* Vols. 1–12, New York: Macmillan: Pittsburgh: University of Pittsburgh Press; Washington, D.C.: Congressional Quarterly.

Key: All figures are based on incumbents' percentage of two-party vote, relative to national party mean. Additional minor impacts are excluded from the table. 1966–68 to 1974–76 incumbent safety averages in the last column have been computed by weighting safety levels for latter year of each election pair by number of incumbents running.

*Actual safety levels.

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[4] There are two other minor sources of change for some of the election pairs: cases where a freshman who defeats an incumbent decides not to run again two years later, and cases where incumbents move from election in multi-member districts (which are excluded from the calculations of safety performed here) to election in single-member districts (e.g., the two New Mexico Democrats who shifted from at-large contests in 1966 to elections in separate districts in 1968). Both of these changes are of very low magnitude since they involve only a handful of cases, and they accordingly will be excluded from Table 3.

1964–66, however, it is clear that the sharp increase in overall safety which we noted above is entirely due to the sizeable .018 value of the veteran incumbent component. The generational replacement term at the same time actually is well below its previous .001 mean because so many of the freshmen in the 1966 election are switched-seat Democrats receiving lower margins than those normally enjoyed by incumbents of their party.

Subsequent improvements in safety after 1966 result almost entirely from substantial growth in the magnitude of the generational replacement component, which exerts a cumulative .041 impact on incumbent margins over the last five election pairs.[5] Generational replacement, though, fails to have a positive effect in 1970, when only 38 freshmen—the fewest in any of the elections considered here—run for reelection, and in 1976, when, as in 1966, a sizeable number of switched-seat, first-term Democrats attain relatively low reelection margins. The veteran incumbent component of change from 1966–68 to 1974–76 acts to depress overall safety, having a cumulative −.023 impact, while the component for incumbent transitions between uncontested and contested election status continues to exert only a slight influence.

Further highlighting the critical role of generational replacement since 1966–68, the last column of Table 3 indicates the average incumbent margin which would have resulted in the latter year of each pair if the new freshman cohorts had been no more electorally proficient than the old. Here we have substituted for the actual values of the generational replacement components the mean .001 value recorded over the 1952–54 to 1962–64 baseline period. The consequent .120 average incumbent margin which would have materialized over the five elections from 1968 to 1976 is fully .027 below the actual average and only modestly higher than the .114 average which existed over the 1952–64 period of unchanged incumbent margins. It seems unlikely that this slight additional safety which would have occurred without enhanced generational replacement effects could have stimulated much analytical concern among congressional researchers.

Conclusion

This study has confirmed Fiorina's speculation that the rise in incumbent safety results from generational replacement. No general pro-incumbent trend has emerged; rather, incumbents in the aggregate have become more secure only because freshmen elected since 1966 have derived greater electoral rewards from their first term.

We have not addressed Fiorina's additional contention that the new freshmen's electoral strength stems from enhanced constituency service, nor have we investigated other techniques which they might have employed advantageously. Our results do suggest, however, that they have not merely been the fortuitous beneficiaries of behavioral change in the electorate. If decline in the salience of partisan cues alone were the cause of stronger backing for recent freshmen, why, then, would voters not have moved toward greater support for old-generation incumbents as well? It thus seems that new members themselves have induced voting shifts in their favor by more effectively capitalizing on the opportunities of incumbency.

Appendix

The technique used to compute the electoral impacts produced by inter-election movements of veterans, generational replacement, and transitions between contested and uncontested election status may be demonstrated through the following example for the 1952–54 election pair. The average .109 margin for the 313 incumbents facing major party opposition in 1954 may be disaggregated into four terms representing the electoral contribution of the following groups: 225 veterans who previously had contested reelections in 1952, 20 freshmen who had defeated incumbents in 1952, 50 freshmen who had won open seats in 1952, and 18 veterans who had no competition in 1952. Similarly, the .119 average margin for the 286 opposed incumbents in 1952 can be disaggregated into a term for the same 225 veterans as above, a second for the 20 incumbents who lose in 1952, a third for 24 incumbents who retire after winning for the last time in 1952, and a final one for 17 veterans who then run unopposed in 1954.

The resulting equation for 1954, then, is

$$313(.109) = 225(.117) + 20(.057) + 50(.074) + 18(.159),$$

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[5] The percentage of all freshman reelection bids involving those who have replaced members initially elected in 1966 or later remains low until 1976–8.1 percent in 1970, 28.8 percent in 1972, 20 percent in 1974, and 54.8 percent in 1976.

which can be written as

$$.109 = [225(.117) + 20(.057) + 50(.074 - .109) + 18(.159 - .109)] / 245.$$

Here, we have expressed the .109 average incumbent margin in 1954 in terms of the contributions provided by the 245 total districts which also had opposed incumbents running in 1952, plus the contributions relative to the 1954 incumbent average of the 68 total districts which did not have any opposed incumbents running in 1952. Similarly, the equation for 1952 is

$$286(.119) = 225(.121) + 20(-.028) + 24(.124) + 17(.256),$$

or

$$.119 = [225(.121) + 20(-.028) + 24(.124 - .119) + 17(.256 - .119)] / 245.$$

Subtracting the 1952 terms from their 1954 counterparts yields

$$-.010 = [225(-.004) + 20(.085) + [50(-.035) - 24(.005)] + [18(.050) - 17(.137)]] / 245,$$

or finally,

$$-.010 = -.004 + .007 - .008 - .006.$$

This final equation decomposes the overall .010 decline from 1952–54 in average margins into four different sources of change. The first two are straightforward, representing a .004 fall-off due to vote decreases among veterans, and a .007 increase resulting because the average 1954 margin of freshmen who had defeated incumbents two years earlier is larger than these losers' average 1952 margin. The .008 drop of the third term reflects the inter-election retirement effect; the relative contribution to 1954 incumbent safety of freshmen who had replaced retiring incumbents in open seats two years earlier is less than the relative contribution to 1952 safety of members who then choose not to run again in 1954. Generational replacement effects will be estimated by summing the second and third terms, which together measure the electoral impact caused by the replacement of veterans by new freshmen. The final fourth term is interpretable as a .006 decline which occurs because the relative contribution to 1954 safety by members who were unopposed in 1952 is less than the relative contribution to 1952 safety by those who then move to unopposed status in 1954.

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COMMUNICATIONS

ON ARTICLES

Reply to Crosby
(Vol. 73, March 1979, pp. 103–12)

Editor's Note: The following reply was not received in time to be published with Crosby's article. In accordance with *Review* policy, it was scheduled for publication in the next issue with available space. Faye Crosby responds. James Davies was also invited to comment on this exchange.

TO THE EDITOR:

A recent article in the *Review* by Faye Crosby (1979) has taken issue with our article (1977), "The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory." Crosby raises two primary and then several minor objections. Her primary objections are: (1) we fail to define relative deprivation and in so doing misrepresent relative deprivation theory; and (2) we employ inadequate measures of what she calls "felt deprivation."

We welcome Crosby's interest in our research and the opportunity to respond. We shall deal with her criticisms in the order she has established, looking at the primary criticisms first and then at the minor criticisms.

Relative Deprivation: A Theory in Search of a Definition

We find Crosby's concern with definition rather strange; indeed, it becomes even more incomprehensible in the light of the complete corpus of Crosby's own work (1976, 1979; Cook et al., 1977). Her criticism of our work implies that there is an agreed-upon definition of relative deprivation discernible from the literature, that it is empirically applicable to the study of collective violence, and that we sorely erred in not using it. As she and her colleagues (Cook et al., 1977, p. 308) have rather explicitly noted, "Examination of the past literature reveals no single definition common to the major theorists. Indeed definitions remained so loose that it is currently unclear as to whether relative deprivation is to be considered a hypothetical construct or an intervening variable." Frankly, we are compelled to confess that we do not know either.

Undoubtedly Crosby felt we should have resolved this elusive problem before moving ahead with our research. Aside from the fact that more agile minds have been unsuccessful in this pursuit, we are compelled to note that it was not what we set out to do. In fact, it was not even appropriate for us to have attempted it. Our concern was to test Davies' (1962, 1969) conceptualization of relative deprivation as he applied it to the black urban riots. To have used another definition would hardly prove commensurate with the requirements of validation.

Having faulted us for not engaging in what appears, from a reading of two decades of research, to have been a rather fruitless venture, Crosby further alleges that we never tested relative deprivation theory because Davies' theory is not a form of relative deprivation. It is discernible from this assertion that although no one presumes to agree what relative deprivation theory is, some know what it is not. Crosby thus alleges that whether the J-Curve is an illustration of relative deprivation, in its progressive form, is "a matter of some debate" (Crosby, 1979, p. 6).

Although Crosby does acknowledge that Gurr (1970) clearly sees the J-Curve as a form of relative deprivation, it is alleged that other sources, namely, Geschwender (1964) and Cook et al. (1977) see the matter in other terms.

A careful examination of Geschwender's (1964) somewhat questionable application of "objective" data to five hypotheses concerning what he calls "the Negro revolt" reveals quite a different story. The question of whether Davies' theory was or was not an example of relative deprivation did not concern Geschwender, for he was concerned only with incorporating under a common rubric those hypotheses he was *unable* to reject. Davies' theory was rejected in Geschwender's analysis. Consequently, he did not concern himself with the question of where Davies' theory fit.

As curious as the inapplicable citation of Geschwender's work is, it is exceeded by Crosby's citation of her co-authored work (Cook et al., 1977), for there is no mention of Davies, neither in the article nor in the references. Consequently, one is compelled to conclude that Davies' work is controversial simply because Crosby alone has alleged that it is. And the reason for this exorcism of Davies from relative deprivation theory becomes apparent when we closely examine another of Crosby's works (1976).

The thrust of Crosby's work has been to establish a precise conceptual definition of relative deprivation. Such attempts at conceptual clarification are laudable. However, what Crosby fails to appreciate is that concepts like language mean what the users have come to make them mean through usage. Science is to no small extent what scientists do, and concepts derive meaning from their actual formulation and measurement as part of the research process. Crosby's work, until very recently, has had no empirical basis, and it comes as a myopic intrusion, ignoring the ongoing research process.

Crosby parts company with Davies for two reasons: (1) he did not posit that individuals must "feel" entitled to possess the item of which they have been deprived; (2) Davies did not deal with the issue of feasibility, i.e., did the individual feel the attainment of the desired commodity was feasible? (It will be noted that it is the absence of an adequate measure of felt deprivation which caused Crosby to take issue with our work, and we will address this concern below in some detail.)

These issues may be important to Crosby but they were obviously not important to Davies. As we were dealing with Davies' conceptualization of relative deprivation and not hers, the issues did not interest us. Crosby may feel that the avoidance of these issues should deprive Davies of his recognition as a relative deprivation theorist and similarly deprive us of any claim to having tested relative deprivation theory, but we doubt whether many will take such autocratic assertions seriously.

Equally irrelevant is Crosby's criticism that we did not properly test Davis' (1959), Runciman's (1966) and most of course her own theories of relative deprivation (Crosby, 1976). In fact, why restrict us to these? Should we have tested every version of relative deprivation theory? Indeed, we are somewhat dismayed that Crosby should require the testing she proposes. In a recent paper (Crosby and Bernstein, 1978), as far as we know, her only effort to subject her attempts at conceptual clarification to empirical verification, her work received only partial confirmation and Runciman's work was disconfirmed. On the other hand, Gurr's (1970) model, which she says we tested, was confirmed.

We are thus accused of not incorporating into our analysis relative deprivation models which were irrelevant to our concerns, which were, until very recently, only conceptual formulations without empirical verification, and which, upon being subjected to empirical testing, have been generally found to be without much substance. This is hardly a sin of omission worthy of intellectual concern.

The crux of Crosby's quarrel with us is really that her understanding of relative deprivation differs from ours and that disconfirming Davies does not disconfirm relative deprivation theory. Her quarrel about the understanding of relative deprivation theory only brings into focus what everyone working in the area sooner or later recognizes, namely, that after at least two decades of researching, conceptualizing and theorizing, social scientists have not reached a commonly agreed-upon theory of relative deprivation or even a consensus as to what the term actually means.

Despite her attempts at resolving this issue, Crosby in her own way has contributed to the confusion. She has set standards without due regard for the ongoing empirical research, or for the realities of data which of necessity must be generated outside the laboratory setting. She has abrogated to herself the role of final arbiter of the dispute, proclaiming that relative deprivation theory is what she says it is, and she has done so while failing to acknowledge the fact that even her empirical research belies her own conceptualization.

Her quarrel is based on our not having set out to do what she has done, or, for that matter, not having seen what she had done as remotely relevant to our work. Our concern was not to fashion a new or even more precise conceptualization of relative deprivation theory but to test its application by Davies to the black urban riots of the 1960s. We were only interested in the overall conceptual problems of relative deprivation theory (with one exception which we shall note below) as they related to this specific case. It is possible when taking our language out of context and removing it from the overall focus of our concern to suggest a more general inquiry into relative deprivation theory. But we do not think this is a fair assessment of our work.

The one area where we did look at a general aspect of relative deprivation theory was in the relationship between current need achievement and future expectations. We did say that the relationship was virtually axiomatic to "all" forms of relative deprivation theory. This was too strong a statement.

What we should have said is that the relationship is important for many forms of relative deprivation theory, especially those which have been applied to civil violence.

Although we are grateful to Crosby for having pointed out this overstatement, we feel that she has confused the issue, having created an even less appropriate overstatement in the opposite direction. As Crosby notes (1979, p. 12), "In plain and simple terms, relative deprivation theory does *not* (emphasis hers) assume a correspondence between present conditions and future expectations." Crosby is wrong! She has failed to understand that the application of relative deprivation theory to collective violence has of necessity concentrated on a dynamic process. As Gurr (1970, p. 46) notes in describing this dynamic process of relative deprivation, "Dynamic analysis requires conceptual tools that take account patterns of change in value capabilities over time." In such analyses, future expectations have been either explicitly or implicitly seen as a result of current need fulfillment (Davies, 1962; Gurr, 1970, p. 74). In fact, Fierabend et al. (1969) go so far as to explicate graphically this very assumption. As we noted (Miller et al., 1977, p. 977, n. 25), Gurr thinks this assumption is so fundamental to the application of relative deprivation theory to the study of collective violence that he and Raymond Duvall undertook research to make the relationship even more precise. Crosby's interpretation of relative deprivation may not assume a correspondence between present conditions and future expectations, but this is hardly the basis for alleging that relative deprivation theory does not make such an assumption.

The Issue of Felt Deprivation

The second of Crosby's major objections is directed against what she calls our inadequate assessment of "felt" deprivation. It will be recalled that it was this failure that she noted in her assessment of Davies, and one which caused her to excise him from the field of relative deprivation theorists. We happily followed Davies' lead in this matter, for it is, after all, his theory that we were testing.

We are unable to ascertain what is gained by Crosby's insistence on defining relative deprivation as an emotion roughly synonymous with a type of anger, dissatisfaction, resentment or grievance. This hardly contributes to the explicit and succinct definition of relative deprivation for which Crosby continues to call.

The issue of "felt" deprivation of course is not unrelated to the issue of how one measures relative deprivation. If relative deprivation is described as a cognition (as we do, following Davies) it will tend to be assessed through perceptual measures of achievements, expectations and gratifications. On the other hand, if one conceives of relative deprivation as Crosby does—as an emotion—then it will be measured through feelings of anger or resentment. But from Davies' perspective, and incidentally Gurr's, these emotions are clearly consequences and not causes of relative deprivation.

This disagreement leads to Crosby's objection to our measures, which she describes as "clear indicators of one of the hypothesized preconditions of felt deprivation" (1979, p. 17). What she apparently is unable to comprehend is that neither we nor Davies was concerned with whether blacks "felt" deprived, resentful, or frustrated. After all, people can feel deprived, resentful, or frustrated for a variety of reasons. Our concern was to assess whether or not the black population experienced, over a number of years, a pattern of relative deprivation illustrative of the J-Curve. Our three perceptual items measured fundamental components of relative deprivation theory—expectations, perceived gratifications and perceived attainments—as a means of assessing the J-Curve pattern. Their ability to measure felt deprivation was completely irrelevant to our concerns, as they were irrelevant to Davies' concerns.

Rejoinder to Crosby's Small Critique

The best that can be said of Crosby's small critique (1979, pp. 109–10) is that it couples casual reading with serious misunderstanding; much of the latter would have been eliminated had she paid more attention to our work. Her questions about the sampling procedure are illustrative. We plainly noted that our sample came from the University of Michigan Survey Research Center election studies. That rather simple knowledge should have answered Crosby's questions about design, comparability of sample size and grouping by region.

While Crosby's attempts at theoretical criticism are maladroit, her attempts at methodological criticism show a naivete about even rudimentary principles of data analysis. Thus her assertion that we should have aggregated our data rather than Davies' data in order to compare the differences between individual and aggregate data is astounding. Davies' (1969) aggregate data came from the U.S. census; consequently, they were far and away superior to any aggregation of sample data. To have tested the aggregate characteristics from our sample data would have obviously been a less accurate, less meaningful and less appropriate test. Indeed, if Crosby were serious about such a test, she should have performed it. As Bross (1960) has noted, the appropriate role of a critic is not simply to cast assertions but empirically to test the validity of counter-hypotheses. Obviously, Crosby preferred not to do this.

This erroneous methodological assertion is followed by another. Crosby conducts a heterogeneity-of-variance test to determine if the pattern of our curve is other than would be expected by chance. The nature and shape of a curve cannot be tested by an analysis of the statistical significance of the individual points of the curve. This is a common methodological error but its frequency of occurrence does not make it correct. We take little comfort in Crosby's observation that our work received some modest degree of confirmation by this inappropriate procedure.

Some Additional Confirmation of Our Findings

We find nothing in Crosby's work to cause us to reconsider our substantive findings or our conclusions. The progressive form of relative deprivation theory does not explain the urban riots of the 1960s. Our data do suggest that the ambiguity and uncertainty that blacks perceive about their current and future financial well-being may have created the conditions for the riots.

To test further the comparative ability of these theories to explain the black urban riots, we conducted some additional testing. We view these tests as supplementary to the ones presented in our research. They are less direct and look at a static rather than a dynamic process. Consequently, they are less powerful, but their value resides in the *additional* confirmation that they provide.

We took "hostility toward whites" as a dependent variable and looked at seven composite explanatory variables. The data were derived from the 1968 Michigan Survey Research Center Election Study. The dependent variable was the SRC feeling thermometer (degree of feeling toward whites). The independent variables consisted of social uncertainty; personal uncertainty; political cynicism; personal political efficacy; system unresponsiveness; status uncertainty; government trust; and relative deprivation. The relative deprivation index consisted of a composite of three other composite variables. These consisted of: (1) individual relative deprivation, a measure that scored relative deprivation when an individual indicated that he anticipated the future to be worse than the present; (2) an individual's present relative deprivation compared to that of blacks as a group, a measure which assesses the respondent's comparative perceptions of these two phenomena; (3) expectations of individual future deprivation relative to the future status of blacks as a group. A complete description of all the measurements and tests for item intercorrelation can be found in Bolce (1976, pp. 180–83).

It should be noted that Crosby (1979, pp. 18–19), after some misinterpretation of Davies, suggested a measure of Davies' operationalization that would incorporate two perceptions: (1) respondents who see the current year as better than the year before *and* (2) those who expect a deterioration of conditions in the future. The operationalization of relative deprivation described above incorporates the notions suggested by Crosby as well as group and individual frames of reference.

These indices were placed in a stepwise regression with hostility toward whites as the dependent variable. The data from this procedure, shown in Table 1, are presented in rank order, in terms of their ability to explain variance in the dependent variable. Not only is relative deprivation the weakest explanatory variable but also its contribution is negative. Personal uncertainty was found to explain more variance than any other variable.

These data are consistent with our earlier findings which illustrated that relative deprivation was not an explanation of the black urban riots. We stand by those results.

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Table 1. Coefficients for Stepwise Regression with Hostility towards Whites as Dependent Variable, Northern Black Sample, 1968

Variable	Multiple *R*	*R*-Square	*RSQ* Change	Simple *R*	*B*	Beta
Personal Uncertainty	.228	.052	.052	.228	.436	.235
System Unresponsiveness	.271	.073	.021	−.099	−.489	.333
Political Cynicism	.348	.120	.047	.149	.453	.218
Personal Political Efficacy	.390	.152	.032	.164	.342	.193
Government Trust	.405	.163	.011	.110	.187	.113
Status Uncertainty	.407	.165	.001	.014	.161	.080
Relative Deprivation	.408	.166	.001	−.032	−.086	−.051
(Constant)					1.186	

Source: Louis H. Bolce, "Urban Violence and the Culture of Ambiguity: The Fluctuation Uncertainty Thesis," (University of Cincinnati: unpublished dissertation, 1976), Table 5:3, p. 158.

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Rejoinder

TO THE EDITOR:

Thank you for the invitation to respond to the communication by Miller and Bolce. I shall divide the reply into three sections: (1) restatement of my general position, (2) examination of a series of specific issues, and (3) musings.

General Issues

Judging from the first paragraph of their communication, Miller and Bolce did not clearly understand my central point. Allow me to reiterate it here. The point of my article (Crosby, 1979) was that Miller, Bolce, and Halligan (1977) were not logically correct in rejecting relative deprivation theory as the explanation of black urban unrest on the basis of their data and analyses. I did not claim that relative deprivation theory provides the correct explanation. Nor did I claim that the J-curve theory may not be considered to constitute one form of relative deprivation theory. Rather, I asserted that the data they presented *did not lead logically* to a refutation of relative deprivation theory. Their claim to the contrary appeared to spring from a lack of familiarity with the relative deprivation literature and from a looseness of operationalization.

My position is, I think, supported by the Miller-Bolce communication. In several instances (e.g., paragraphs 4, 11, 14, 16, and 19, pp. 818-20), Miller and Bolce announce that, in their 1977 article, they were concerned only with Davies' J-curve theory. Obviously, the J-curve is one formulation of relative deprivation theory. Because they were concerned with only one version of the theory, they were prevented, logically, from drawing conclusions about the theory as a whole.

Specific Issues

The Miller-Bolce letter raises numerous issues. I would like to discuss nine of them, taking them in the order of appearance in the Miller-Bolce communication.

1. Miller and Bolce state in their opening paragraph that one of my "primary objections" to their article was their failure to define relative deprivation. In fact, the definitional failure was but one example of what I took to be the broader problem: their misrepresentation of the literature. Other aspects of the problem in my view included their equating J-curve and relative deprivation theory, their ignorance of the work of Davis (1959), Runciman (1966), and myself (Crosby, 1976), their slanted view of Gurr (1970), and their false claims about the empirical research performed in the area of relative deprivation.

2. The communication accuses me of claiming that "Davies' theory is not a form of relative deprivation theory" (p. 818). In fact, I said that it is one form of relative deprivation theory. My claim was simply that Miller et al. had mistaken a part for the whole. In my article, I stated it this way: "Arguing that J-curve theory is invalid is one matter. It is quite another matter to argue that because J-curve theory is invalid, all of relative deprivation theory is invalid." (It is, incidentally, my belief that Miller and Bolce fail to demonstrate that the J-curve is invalid. Davies [1978] points out some of the ways in which Miller et al. present a "deficient" test of the J-curve hypothesis.)

3. The tenth paragraph opens with: "Crosby parts company with Davies. . ." (p. 819). It seems that Miller and Bolce are referring here to some parts of an unpublished manuscript which I sent them (Crosby and Bernstein, 1978). I am confused, however, by the reference to Davies. Bernstein and I discussed Davis, not Davies.

4. Miller and Bolce suggest that the only problem I found with their view was that it did not agree with my own. Among the sentences one could cite are these: "The crux of Crosby's quarrel with us is really that her understanding of relative deprivation differs from ours. . ." (p. 819), and, "Her quarrel is based on our not having set out to do what she has done" (p. 819). It seems ironic indeed that Miller and Bolce give me and my formulation of relative deprivation proportionately much more attention than I myself did.

My original piece did not claim that Miller, Bolce, and Halligan ought to know my model of relative deprivation. On the contrary, I explicitly excused them for not knowing it. I did chide them, however, for not knowing the work of Runciman and of Davis. I remain dismayed by their continued silence about these scholars. Runciman and Davis deserve more than the passing glance they receive in the Miller-Bolce communication. On p. 819, Miller and Bolce characterize my concern for Runciman and for Davis, and (by implication) for Gurr as "irrelevant" and ask if they should be expected to test all the relative deprivation theorists. The answer is obvious. If one wants to make claims about relative deprivation theory in general (as they did in 1977), then one must give the theory a fair hearing. No part can stand for the whole.

5. Miller and Bolce continue to assert that relative deprivation theory assumes a correspondence between present conditions and future expectations. To support their claim that Gurr sees the present-future link as "fundamental to the application of relative deprivation theory to the study of collective violence" (p. 820), they refer to Gurr's ongoing work with Duvall. Concerning the Gurr-Duvall collaboration, the 1977 article read: "All we can say is that sometimes, and for some groups, future expectations are a function of current need fulfillment, and at other times, for other groups, this relationship does not hold" (Miller et al., 1977). Which tune are we to follow?

6. On the issue of "felt deprivation" Miller and Bolce seem off the mark. If they accorded Davis and Runciman the attention they deserve as relative deprivation theorists of long standing, Miller and Bolce would have to admit that deprivation is conceived as a feeling. Even Gurr speaks of a tension state. Davies too mentions emotions. One distinction that Miller and Bolce make is rather too subtle for me. They show no interest in "whether blacks 'felt' deprived" and restrict their curiosity to "whether the black population experienced . . . a pattern of relative deprivation" (p. 820). I do not know how one can experience a pattern. Nor do I follow how one can separate feelings from experiences.

7. In my article I suggested that Miller et al. ought not to have drawn conclusions based on a comparison of their Table 1 and their Table 2. We see from their communication that the former contained aggregated census data; the latter contained individual-level survey data. Whether the census data are superior or inferior to the Michigan survey data is irrelevant. The point is that they are different. This means that the observed differences between Tables 1 and

2 could have been owing to (a) the different levels of data (aggregate-individual) as Miller et al. claim or (b) something about the way in which the data were collected. The alternative is plausible.

8. Miller and Bolce believe that I made a mistake in my own small statistical analysis. They claim that one may not test the nature of a curve by analysis of the individual points. At least one standard statistical textbook (Hays, 1963) suggests the opposite; Miller and Bolce thought I had tested to see if the "curve is other than would be expected by chance" (p. 821). I did *not* test to see if the curve for northern blacks differed from chance variations. I did test to see if the variance in that curve differed significantly from the variance in the curve for the northern whites. Their own original uncertainty argument rested on their "eyeball" observation that the northern black curve fluctuated more than the other curves. I was attempting to provide a more precise test of their speculations than they themselves did.

Since writing my original piece, I have learned of an analysis which might be equally or even more appropriate for the issue of fluctuations than is the test for heterogeneity of variance. It is Tukey's test for smoothing curves (Tukey, 1977, esp. chapter 7). To use this test, one would have to convert the aggregate data into single points, as I did in the test for heterogeneity of variance. The advantage of Tukey's test is that it is sensitive to dips and ridges, while the standard test for heterogeneity of variance is not.

9. I am interested by the dissertation data analysis presented by Miller and Bolce at the end of their letter. They claim the operationalization of relative deprivation, described on p. 821, "incorporates the notions suggested by Crosby as well as group and individual frames of reference" (p. 821). As far as I can tell, the measure does get at two of the hypothesized preconditions of my model (Crosby, 1976). But what has all this to do with assessing Davies' (1962, 1969) rise and fall model with which it is linked in the text?

Musings

Let me reiterate a point. Neither in my original article nor here do I wish to claim that relative deprivation theory provides the best explanation of black urban unrest. My central point is simply that the data and the analyses in the Miller, Bolce, and Halligan article and in the Miller-Bolce communication fail to refute relative deprivation. Why, then, all this fuss?[1]

One reason for the fuss, I think, has to do with the nature of social scientific inquiry. What differentiates scientific and logical endeavors from other endeavors is a matter of method and not of conclusion. One may conclude in the next years or months that relative deprivation theory is wrong or that it is right. Quite apart from the conclusion itself is the method one uses to reach the conclusion. The scientific method involves precision and logic. In the end, then, my concern with the work of Miller et al. rests on my conviction that in the social sciences we ought to care about how we reach our conclusions as much as we care about the conclusions themselves.

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[1] That my own fussing has been tempered considerably is due in large measure to Robert Abelson, Travis Crosby, and especially Donald Kinder. I am grateful for their help.

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Comment by Davies

To the Editor:

As the accused and admitted parent of a theory that helps explain political violence, I want to say something about how the idea was conceived, its gestation, and about its continued growth since it was born in 1962. The idea, in its original or improved versions (Davies 1962, 1969), has been published in whole or in part more than 25 times, but the occasional abuse and misunderstanding of this brainchild compels a parental defense. An earlier analysis in this *Review* (Davies, 1978) of the critiques by Miller, Bolce, and Halligan (1977, 1978) of the J-curve seems not to have established clear contact with the critics. At least, they have not very explicitly responded to my queries about what they think black people wanted in their 1960s rebellion and about when cycles begin.

The Ancestry and the Gestation

The J-curve's ancestry is a bit complicated but clearly traceable, at least back to the second generation. The idea was conceived about 1956, when I was trying to figure out why the 1894 strike of Pullman railway car workers took place when it did. Census Bureau data on both wages and prices for the three decades following the 1861–65 Civil War revealed a steady, slow rise in real wages among working people. Wages remained roughly constant; prices declined. During the 1894 recession, the gradual, generation-long rise in real wages was interrupted abruptly, most particularly for the Pullman railway car workers. Many people who worked in the Pullman shops and lived in the town which the company had built to house them were put on part-time schedules or were laid off altogether. Many of these people had very little to eat and were simultaneously pressed by the company to pay their suddenly delinquent rents. A nice J-curve describing the generation-long event popped out of the data, into my head, and onto the page of notes I was making.[1]

The idea grew and was nourished by the writings of some rather remarkable people, whom I charge with grandpaternity: Crane Brinton's *Anatomy of Revolution,* which I had read when it was first published in 1938; the work of Abraham Maslow, whose need hierarchy (1943) is a basic component of the theory and the research of now several political psychologists (Davies, 1963; Aronoff, 1967; Knutson, 1972; Renshon, 1974; Inglehart, 1977; and Burns, 1978). And there were two remarkable others, whose most relevant work I encountered after first plotting the J-curve.

Tocqueville's 1856 analysis of what led to the French Revolution noted in grand detail how things in France steadily improved during the eighteenth century, not just culturally but also economically and socially. (From Brinton and others I'd already got information about the 1788 economic crisis, which included a critically sharp drop in the supply of grain.) To this was added rumination about Marx and Engels' 1848 *Communist Manifesto* which hammered on the ways industrialization was making the life of factory workers steadily worse. In the spring of 1960 Seymour Martin Lipset pointed out to me that Marx had written not only about increased immiseration but also about how people compare their circumstances with those of people in other classes: "Our desires and pleasures spring from society.... Because they are of a social nature, they are of a relative nature."

Three major components of theorizing about revolution were thus related to the writings of Tocqueville and Marx and Engels—jointly and separately. Tocqueville described an upswing before revolutions, Marx and Engels a downswing. And Marx (1849) clearly stated, in one of its forms, what Stouffer (1949), Pettigrew (1967), and Gurr (1968, 1969 and 1970), long after, called relative deprivation.

Stated most simply, the theory that was conceived about 1956 in a study of the Pullman Strike, nourished in utero mentis by the ideas of Brinton, Maslow, Tocqueville, Marx, and Engels, and born in 1962, states this: when a relatively long period of steady rise in what people want and what people get is followed by a short period of sharp reversal, during which a gap suddenly widens between what people want and what they get, the likelihood of revolution increases sharply.

The Growth of the Idea Since 1962

The Black Rebellion and the Student Rebellion of the 1960s rekindled interest among social scientists in pursuing nonsuperficial explanations for these very significant events. The timely appearance of Ted Robert Gurr's *Why Men Rebel* in 1969 provided a solid basis for deepening the study of conflict. Unfortunately, people who undertook such new research adopted the practice of not really seeking out the origins of ideas about civil violence, in the writings either of the honored but ill-understood dead–Aristotle, Hobbes, Tocqueville, and Marx and Engels–or of more recent writers like Sorokin (1925), Edwards (1927), Pettee (1938) and Brinton (1938, 1965).

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[1] The data on real wages are in U.S. Bureau of the Census (1949, pp. 66, 231–32, 235). See also Lindsey (1942, esp. Chs. 5 and 7).

An even greater misfortune is that such recent researchers have shown little understanding of basic human motivation as theory and research about it have developed in recent decades, out of the work of James (1890), McDougall (1908), Freud (1930), Murray (1938), Fromm (1941), and Maslow (1943). Like John Locke, Karl Marx, Ivan Pavlov, J. B. Watson, and B. F. Skinner, recent researchers seem to suppose that human beings are (almost) entirely what society conditions them to be. Or that, if they do innately possess anything, it's only the animal desires to survive and procreate.

In brief, the unexamined and seemingly Marxist assumption as to human motivation has been that humans do indeed, like animals, live by food alone. Even this assumption is falsely founded in Marx, whose concern was not simply with the extreme physical deprivation which poverty produces but also with the ways in which poverty breaks the elemental social ties of family *and* destroys human dignity *and* makes it impossible for people to fulfill their uniquely human potential. Researchers in civil conflict in the 1960s and 1970s have exhibited little theoretical understanding of earlier writing on either revolution and rebellion or of basic human motivation, because they evidently have not read anything relevant that was written before the 1960s. In Arthur Koestler's phrase, they are a generation without an umbilical cord.

In addition to innocence regarding motivational psychology, there has been another deficiency in recent research in political conflict: a non- or misunderstanding of time periods and therefore, inter alia, of what is and what is not a complete cycle. This deficiency becomes particularly apparent when the theory that is being examined relates to cycles within cycles and epochs within epochs. The deficiency makes it possible to deny sequence (by starting in the middle of a cycle) and to deny cause and effect (by discarding the early part of a cycle, during which the causes are starting to produce effects).

Let me give an example of cycles within a cycle, the ones I mentioned in the 1962 article, in discussing the Russian Revolution. The emancipation of the serfs in 1861 began what I regard as the final long upswing of expectations that peaked 44 years later, on the eve of the 1905 revolution. The long cycle ended its downturn 56 years later in February 1917 when, in the midst of war, a general strike and a revolution "spontaneously" burst forth. After this February event, the government never again was able to regain control and restore order. The 56-year J-curve cycle was complete.

Within this long cycle, I included briefer, less intense J-curve cycles, with sharp downturns following long rises. The first briefer cycle was related to the assassination of Alexander II in 1881. The second came when the futile war in East Asia was followed by the 1905 Revolution. And the third came when the solid recovery after 1910 was followed by the brief downturn which the German attack precipitated in 1914.

The three short cycles have to be understood in context of the great cycle. No one of them was big enough to effect the final overthrow of the Tsarist regime, but their effect was cumulative. Widespread rebellion burst forth in 1905, even though serfdom had legally ended 44 years before, promising to make one-time serfs better off and thereby raising their expectations. Factory workers by 1905 were very likely better off than they had been as either serfs or the children of serfs. When a variety of such improvements had accustomed people to expect continued improvement, the degrading and wasteful war against Japan precipitated the sudden downturn.[2]

The phenomenon of cycles within cycles can be better understood by noting its comparability to ocean waves along shorelines. Small waves have a length of a few inches and a height of less than an inch. Larger waves, up to and including those that result from severe storms or earthquakes, have a length ranging from several yards to several miles and a height ranging up to several yards. The energy carried in these large waves is enormous. Similarly, wave lengths in the electromagnetic spectrum range from tiny fractions of a micrometer, measured in angstrom units, to several meters;

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[2] See Davies (1962, pp. 10–13) for more detailed appraisal of the waves-within-waves. (Note particularly Figure 3, p. 13.)

amplitudes, like the height of ocean waves, similarly vary greatly. And of course in both kinds of wave, there are cycles within cycles—shorter waves within longer ones. These cycles within cycles provide the means by which music is heard and, separately, the means by which it is broadcast by radio.

Theorists and researchers who fail to recognize the cyclical nature of public moods can neither understand the beginnings and turning points of cycles, including J-curve cycles, nor undertake causal analysis of civil conflict. This cyclical characteristic derives from the states of mind of people in a polity, as these mental states change during both long and short cycles and as they form a usually unpremeditated consensus to act at a particular time. Starting points of upswings are unavoidably harder to define than turning points, for reasons of the same kind that make it harder for two people to determine when they started to have a falling out than it is for them to determine when they had a real fight and ended their friendship.

The Rising Expectations of Black People

The recent analysis of the J-curve by Miller et al. (1977, 1978, 1979) in this *Review*, in the context of the Black Rebellion of the 1960s, exemplifies the deficiencies I have mentioned: the lack of understanding of theoretical intellectual history, a reduction of human motivation to material needs only, and a misunderstanding of cycles and therefore of starting points. The expectations of black people that these researchers consider do not include any expectations other than "financial" ones, and they consider these only during the period of their data base (1956–1968). They deprive black people of their history.

What is puzzling is that the authors evidently read, in the 1969 version of the J-curve, my analysis of events that led the expectations of black people to rise after 1940 (Davies, 1969, pp. 716–25). There I specifically mentioned occupational integration into the armed forces and defense industries during the Second World War (a major advance in *both* economic *and* equality expectations), a decline in lynchings, integration in housing and education, reduction of the income-education gap, and the beginning of successful direct action in the 1956 Montgomery bus boycott. And I mentioned the downturn coming after the April 1963 pitched battle between police and lunch-counter "sitinners" in Birmingham, with the first outbreak of major violence occurring in the Watts area in Los Angeles in August 1965. The recent analysis ignores all this and reduces its critique to only that segment of material expectations that relate to finances, not beginning with 1940, when the final big upswing began, but 1956.

When *Do* Cycles Begin?

This misunderstanding of cycles is even harder for me to understand than the lack of theoretical background. If Miller et al. reject the idea that the last upturn commenced among black people in 1940 and assert that it commenced in 1956, they should explain why they ignore such expectation-raising events as the reduction in discrimination in jobs, housing, public transportation and education—all of these events commencing well before 1956. And they should explain why, aside from the fact that 1956 was the year in which University of Michigan Survey Research Center began gathering the data they analyzed, they see 1956 as the year when a rise in expectations commenced.

After listing a set of four hypotheses about the "financial" circumstances of black people from 1956–1968, Miller et al. state (1977, p. 974): "The rejection of *any* one" of their four statements about the financial circumstances of black people after 1956 "would result in a rejection of the J-curve theory as an explanation of the black urban riots." This could be true only if the final long cycle that I said commenced about 1940 actually commenced in 1956 and only if the one thing black people were concerned about was their financial condition. It is hard to imagine a clearer case of the drunk looking for his keys under the street lamp because that is where the light is brightest.

The Development of Theory beyond Mere Description

Having disposed of the J-curve, Miller et al. come up with what they call a fluctuation-change hypothesis (1977, p. 980) whose essence evidently is that civil violence is the product of rather intense ups and downs, whose meaning to those experiencing it is ambiguous. This does make sense, if the authors can establish that the intense ups and downs and the ambiguity preceded, and therefore can be said to have produced, the violence itself. Although there is a time interval between their downs of 1956 and 1962 and what I consider the first major outbreak of concerted black violence in August 1965 (the Watts riots in Los

Angeles), Miller et al. also note a down in 1966. I think what they are doing is predicting a storm by noting the big waves that indeed are the storm itself.

Other theories about civil conflict are akin to that of Miller et al., in that they do not substantially distinguish between the causes of events and the events themselves. Mancur Olson (1963) says that rapid change is a cause of civil disturbances. Indeed there *is* an association between rapid change and such conflict, but does rapid change occur spontaneously? Charles Tilly and associates (1972, 1974, 1978) have proposed a theory, first, that the struggle for power between various segments of society is the cause of civil violence, and, second, that the formation of groups is such a cause for that struggle. Like Olson's, Tilly's theory is more tautological than etiological. That is, the struggle for power is a definition, not the cause of political conflict—of every kind. When the struggle, the conflict, becomes violent, we call the process rebellion or revolution. And what political action—whether peaceful *or* violent—takes place *without* the formation of groups? Group formation is a cause of almost anything in social life.

A basic question in developing theory is its explanatory, that is, its metadescriptive, value: does a theory make it possible to sort out—eventually in advance—causes from consequences clearly enough so that we can distinguish one from the other? The fluctuation-change hypothesis, the rapid-change analysis, and the power-struggle theory all are almost totally situational and inexplicit about time as a dimension. They describe circumstances, and they regard the reaction *within* human beings to these circumstances as either irrelevant or unidimensional. They say, by implication, that it does not really matter what human beings want and what therefore causes tension when they do not get what they want. Or they imply that if they do want something, it is only material goods.

The J-curve was conceived as I studied the Pullman Strike of 1894. Like the Black Rebellion, it was not a full-scale revolution. In the 1962 article, I applied the J-curve to another such event: Dorr's Rebellion in Rhode Island in 1842. Neither event was a full-fledged revolution, but ruminations and readings after looking at the Pullman Strike made it seem appropriate to apply the idea to the big ones. Ruminations and readings since 1962 made it seem appropriate to re-apply the idea not only to the Black Rebellion but also to the Student Rebellion in America in the 1960s: university students whose nonmaterial expectations of being able to prepare for challenging careers were frustrated by the threat of military service in Vietnam. It was not only physical deprivation but also deprivation of the need for self-actualization that helped to generate the Student Rebellion. The Vietnam War caused students thus to experience a rapidly widening gap between what they wanted and what they were getting.

In short, I think the J-curve, though not total, is a rather fundamental theory. It is so because it is based on psychological foundations, in basic theory of human motivations. For the same scientific reasons that nuclear physics takes the atom as a unit of analysis, the theory takes the individual human being as a unit of analysis. Theory that is based on analyzing social change in its own terms and not in terms of the intrahuman causes of social change is tautological and incapable of probing to the depth necessary for social analysis to become more adequately causal. Marxist theory is indeed a necessary part of the explanation of the activation of poor people in developing countries, but it has not worked adequately to explain violence in either developing or advanced countries. For one thing, the continuing conflict between business and labor has become less violent, less revolution-prone with trade union leaders, in many instances, in both capitalist and socialist countries, treating their "men" like commodities, in what Marx said was the capitalist manner. For another, a major component in such upheavals as the 1952 Egyptian and the 1979 Iranian revolutions was the demand for recognition of the equal station in the family of nations that is not a function of either economic or military power.

The analytical situation, in sum, is complex but not impossibly so if one does not wholly ignore human beings not only as units of analysis but also as among the originators—among the causes—of civil conflict. In addition, it helps to look for evidence that large groups of human beings in a society are at approximately the same stage of development and are likely to want to enter the next stage at about the same time. As I saw it, the Black Rebellion of the 1960s focused clearly on the newly shared desire of black individuals to pursue, as a collectivity, their equality and dignity, in American society. They could not have so acted together if they had not already achieved the minimal economic well-being that, for example, meant that they had enough money to go to a lunch counter with the ability to pay for their own lunch and enough money in Montgomery in 1956 to buy and use enough cars to transport one another to work and thereby make the bus boycott effective.

Some New Theory

In two recent articles, I have posited a relationship between the stages of development of individuals and the stages of development of polities (Davies 1977a, 1977b). This theory poses more grievous problems of testing than the very real but I think manageable ones relating to the J-curve. This theory elaborates not only on the general political implication of basic human needs but also on how, for large groups of people in a polity, like black people in America in the 1960s, the transition from one stage to the next takes place during the same epoch. The theory also argues that frustrations of physical, material needs are by no means the only ones that produce violent political conflict.

The revolutions in the Third World, late in the twentieth century, against colonial domination are in part an echo of the revolution against colonial domination that commenced in 1775 and was justified a year later with the statement of a first cause: "All men are created equal." In the twentieth century, the same demand is among the foremost of those that have helped to cause such revolutions as that which occurred in Iran in January 1979.

Until the social sciences develop more specific equations which more specifically include human beings as *active* determinants of change—rather than as merely acted-on products of their historic and current circumstances—theory and research will remain radically deficient. To fail to consider the full range of basic human wants—from survival, to social ties, equal dignity, and self-fulfillment—leads to ingenious and/or rigorous exercises which confirm only the obvious: that there is a relationship between instability and violence.

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Comment on Dobel (Vol. 72, September 1978, pp. 958–73)

To the Editor:

Patrick Dobel's ambition in "The Corruption of a State" (1978) is to contribute a general positive theory of corruption. He builds his theory upon the references to corruption he was able to find in the writings of Thucydides, Plato, Aristotle, Machiavelli, and Rousseau. However, while such sources can give his theory academic respectability, they cannot, by themselves, give it scientific credibility, especially since they are quoted outside their historical and, to a large extent, philosophical, contexts. Only internal consistency and external verifiability can give Dobel's theory scientific credibility.

It is often said that corruption is the second oldest human trade, the first being prostitution. But no matter how widespread the practice, the *pattern* of corruption and prostitution varies in time and space. There is a common general idea of what corruption is; but the precise definition of corrupt behavior differs historically and geographically. Indeed, it also differs within the same historical and geographical context. Thus, dealing with corruption, one must refer to two patterns of behavior: the actual behavior of the corruptor and/or the corrupted, and the accusations of corruption that others address to either or both of them in connection with their behavior. For instance, an Italian politician might solicit a bribe from a foreign corporation and be accused by his rivals of allowing his policies to be shaped by foreign concerns; likewise, an American corporation might bribe a foreign politician and be accused by American politicians of jeopardizing American policies abroad. What is corruption?

Dobel proposes an objective definition of corruption: namely, the process of internal decay and disintegration undergone by a society. It is an observer-invented definition and as such must be judged as to whether it throws new light upon the phenomena to which it is applicable and/or withholds potential light from other phenomena to which it can no longer be applied.

Dobel, however, does not label as corruption *all* processes of social decay, but only those characterized by disloyalty. To call such processes corruption is by itself unlikely to throw light upon them. Unless. . .

Underneath Dobel's explicit dissertation runs another theory, based on the popular concept of corruption. According to this other theory, disloyalty and corruption are not the same but different behavior patterns, *and* the second one, corruption, is caused by the first, disloyalty. In other words, Dobel would suggest approaching from a perspective of corruption the study of the processes of social decay characterized by the disloyalty of the members to the group. This is not a banal suggestion and could indeed be a useful one.

Dobel himself, however, deals a blow to the attractive simplicity of this theory when he argues that disloyalty does not necessarily lead to corruption. For such development actually to take place, another factor, inequality, must intervene. Dobel's theory could thus be stated as follows: a social group characterized by inequality among its members *and* members' disloyalty to the group, sooner or later becomes corrupt and eventually disintegrates.

This is, however, just one interpretation of Dobel's never-too-explicit train of thought. Dobel continuously reshuffles the relationship among his concepts. Sometimes, for instance, he establishes the direction of the causal link among them rather differently, as follows: when the members of a social group are unequal among themselves, they are bound to behave disloyally to the group, *that is,* to behave in a corrupt way. Eventually their behavior will lead to the disintegration of the group. Within this logical framework, corruption again becomes another name for disloyalty and thus loses all its explanatory significance.

A new reversal of logical connections in Dobel's theory of corruption occurs when Dobel draws yet another behavioral pattern into it, factionalism. He then argues that factions result from corruption, which in turn results from inequality; factions, in turn, destroy the members' loyalty to the group which thus eventually disintegrates. Indeed, the study of factionalism from a perspective of corruption could be rewarding and the hypothesis that factionalism is a function of corruption worth testing. The hypothesis, moreover, would not require a redefinition and consequent monopolization of the concept of corruption, which would thus remain available for the study of other unrelated phenomena. Indeed, the traditional concept of corruption could benefit from this new application; since its definition would probably need to be broadened and formulated in terms such as the following: the use of ideological and strategic power (i.e., political power in its widest acceptation) to the furtherance of private (i.e., personal and/or factional) ambitions.

Unfortunately, Dobel does not seem fully to appreciate the analytical power of his hypothesis. Taking a moral rather than a scientific viewpoint (corruption is bad, factions are bad), Dobel uses the hypothesis linking factions to corruption as a stick with which twice to beat politicians. Moreover, in his crusading zest, Dobel loses sight of the causal direction of the relationship corruption-factions, and sometimes he writes as though corruption leads to factionalism, sometimes as though factionalism leads to corruption.

The conclusion of the above discussion must be that Dobel's theory of corruption as a whole lacks internal consistency, although it contains some interesting analytical suggestions. Dobel's theory, however, also pretends to be a *positive* theory of corruption and, to this extent, the test of its external verifiability becomes all the more crucial.

Insofar as Dobel defines corruption as the process of internal decay and disintegration undergone by a "just, equal, and stable society," he raises serious questions about the possibility of empirically applying his theory, let alone verifying its validity, since it is arguable that the phenomenon which Dobel's theory is meant to explain has actually never existed and probably never will exist.

A case could, however, be made to the effect that Dobel's theory sets forth the reasons why a "just, equal and stable society" has never come into existence, or, by implication, the patterns of behavior that would need to be stamped out in order that such a society can come into existence. In this respect, Dobel's propositions are not only justifiable but thoroughly commendable. They, however, do not constitute a positive, but a *normative,* theory of corruption; or, better, a normative theory on how to stamp out corruption, stated in a roundabout way. As such, their doctrinal value is probably high, but their scientific value is nil.

F. D. MARENGO

University of Bath

Reply

TO THE EDITOR:

Marengo's comments focus on two criticisms: he alleges, first, that the theory of corruption I have formulated is either tautological or internally inconsistent; and second, that the theory as formulated has normative but no scientific value. I shall address these charges in their order of relative importance.

Marengo's progressive reformulations show a basic misunderstanding of the theory. The confusion lies in his insistence that in my view corruption consists in "disloyalty" or "disloyalty of members to the group." My theory, however, never uses the word "disloyalty" nor does it speak of "groups" in any generic sense. I speak, instead, of "states" or "factions" and attempt to present an interpretive theory of historical phenomena. Marengo raises the theory to a level of abstraction never intended and in my view inappropriate. His simple focus on "the group" presumes a closed and exclusive set of loyalties by which disloyalty becomes easily defined and enables him to charge that for me disloyalty becomes synonymous with corruption. His misappropriation of the concept of "disloyalty" and charge of tautology can best be answered by a recapitulation of the role of loyalty in the theory.

Loyalty is a precondition of civic virtue, and civic virtue encompasses the commitments to and dispositions towards other citizens, symbols and institutions which enable a state to resolve conflicts with a minimum of violence and a maximum of justice and equality. Insofar as a just, stable and equal political order defines the functions of the state and serves as an ideal type in the theory, it is the theoretical background by which other conditions can be evaluated and understood. Civic virtue is a necessary theoretical component of the type; therefore, any realistic expectations of approaching the ends of the state would require the approximation of civic virtue.

Corruption of the state, on the other hand, results as the minimum civic virtue in the state breaks down and leads either to privatization of moral concerns dissociated from public loyalties or factionalizations of loyalty. The importance of loyalty in the theory stems from its status as a necessary condition for civic virtue and, therefore, from its absence or refocusing. Corruption and disloyalty do not collapse into a tautology.

Marengo charges the theory with inconsistency on two counts. First, he argues that in my theory there are unnecessary and reshuffled relations among corruption, inequality and factions. Second, he argues that I confuse the causal direction of corruption and factions, sometimes arguing that corruption leads to factionalism and sometimes the reverse.

I answer that it is necessary to sacrifice "attractive simplicity" to do justice to the phenomena. My theory presents two necessary conditions for the decline in civic virtue through the destruction or refocusing of loyalty and thus, the emergence of the patterns of politics which characterize the corruption of the state as opposed to other forms of corruption. First, by far the most important, is systematic inequality over time (usually intergenerational) in access to wealth, power or status. The second, also significant, is the factionalization of the state. A faction constitutes a specific type of group which possesses internal moral and symbolic coherence and autonomous power. Theoretically, inequality and factionalization are the necessary interrelated conditions and together constitute the sufficient condition for beginning the process of undermining civic virtue and the corruption of the state.

It should be clear that, as with inequality and factionalization, the relation between factions and corruption of the state is an interactive process. As coherent symbolic and moral entities, factions provide alternative foci of loyalty to the state and the common good. The faction's success in using the state for private purposes leads to a decline in the efficacy of the state's symbols and institutions which leads more people to abandon their civic commitments for privatized or factional concerns. These systematic interactions are vital. The theoretical interpretation of historical and political actions or situations (paradigmatically the United States) is not reducible to billiard ball causality (even billiard ball interactions are not unidirectional) except at the expense of trivializing the issue.

Although I appreciate Marengo's comment that the doctrinal value of the theory is high, I do not agree that its scientific value is nil. His claim that the scientific value of a theory rests upon its external verifiability rests upon an overly narrow and simplistic view of science and the role of theory within it. As most contemporary philosophers of science are well aware, theory performs a number of functions in science, and concern with verifiability and strict causality occurs at a fairly low order of analysis. One purpose of my theory and of similar enterprises is to reconceptualize the problem of political disintegration and provide a general language in which to make sense of the phenomena. The theory brings together a number of seemingly unrelated phenomena and reintroduces other aspects, such as the role of civic virtue. It links together moral aspects stressed by conservative critics with the more structural aspects of radical critics. Thus, it creates a new framework of concepts within which to understand the phenomena of political disintegration and it suggests new linkages and multiple interrelated causes in political life which have been ignored up to this point. This reconceptualization is both prior to and distinct from the generation of hypotheses subject to verification. The latter always arise only within the context of a general conceptual theory. The general theory, like many theories even in the natural sciences, is not intended to be "verifiable" in the positivistic sense Marengo seems to want.

Finally, Marengo's attempt to sunder the normative value from the scientific value oversimplifies the nature of a general framework in political theory. The theory could be read as normative discourse upon an everpresent limiting factor in the attainment of a just, stable and equal political order, and it could be judged, with its accompanying prescriptions, upon its consistency, plausibility and fit with one's moral agenda. But a prescriptive political theory makes little sense unless it identifies a real-world problem and why it exists as a normative "problem" rather than merely an issue of academic interest. In my theory, the just, stable and equal state simultaneously serves as an ideal type to help locate political phenomena and as a goal by which we can evaluate the desirability of an actual situation. The normative discourse, therefore, must inform the conceptual framework if the theory is to identify areas which generate the most harm and is to provide a more coherent ordering of prescriptions. I do not expect (alas) to "stamp out" the corruption of the state, but my theory may help us cope with an everpresent tendency.

J. PATRICK DOBEL

University of Michigan, Dearborn

Comment on Ferejohn (Vol. 71, March 1977, pp. 166–76)

TO THE EDITOR:

In the absence of any response from Edward Tufte in defense of his argument (1973) against the criticisms of John Ferejohn (1977), I feel compelled to become a participant in the expanding debate in this *Review* over why competition in congressional elections appears to be declining (or, as David Mayhew and Morris Fiorina prefer to label it, "the case of the vanishing marginals"). While various others, e.g., Robert Erikson, Warren Kostroski, and Walter Dean Burnham, have all been very much involved in this debate, I would like to restrict my comments to Ferejohn's article and, in particular, to his objections to Tufte's thesis that redistricting has made an independent contribution to reduced competition and, hence, increased incumbency of House members.

Ferejohn's objections to Tufte's argument are twofold: (1) it is implausible because there were fewer legal restrictions on gerrymandering before the Court's rulings than there were after its decisions; and (2) Tufte failed to look at changes in the number of competitive districts in states where no reapportionment occurred. If he had, Ferejohn argues, he would have found that the drop in the percentage of competitive seats following reapportionments was not due to redistricting, since the decline occurred in unredistricted areas as well. Ferejohn therefore concludes "that redistricting has had no influence at all on the swing ratio," that "the decline in the number of marginal districts is a general one. . ." (1977, p. 168).

Concerning the first point, I feel obliged to point out that while it is true the courts have somewhat restricted the ability of state legislatures to manipulate congressional district boundaries, it is important to note that this restriction has extended primarily to the number of people that may be included in a district, and has not affected the shape or compactness of a district. I would contend that the ability of state legislatures to gerrymander districts has rarely been impeded by equal population requirements. Equally as important, however, is the fact that reapportionment/redistricting decisions of the Court in the 1960s awakened a large number of "sleeping dogs." In many states the issue of redistricting was largely dormant prior to the *Baker* and *Wesberry* decisions simply because such states refused to redistrict their House seats. For example, at the time of *Wesberry* there were 13 states that had not redistricted since the 1930s or '40s, or even prior to that. Moreover, rather than undergo the acrimony of redistricting after every decennial census, some states that gained a congressional seat chose to leave their existing district configuration intact, and instead chose merely to elect the new Congress member at-large. The point here is that the Court forced the issue by its actions such that nearly every state had to review and reshape its congressional district scheme in the light of equal population requirements, rather than rely upon piecemeal changes, or, as in many cases, the inertia of inaction.

Ferejohn's second point would lead one to believe that since a decline in the proportion of competitive districts occurred in unredistricted states approximately equal to the decline in competitive districts in states that underwent redistricting, the competitive effects of redistricting were nil. No so. If one is to assess the impact of the independent variable "redistricting," the best way to go about the task is not to use such gross comparisons as the proportion of "competitive seats in states that have and have not been redistricted" at two points in time, as Ferejohn has done. What he has tended to confirm, as have others previously, is that there has been a general decline in competitive seats. But what he has not done is to examine the competitive nature of districts in those states that *have* undergone redistricting, and compare the competitiveness of those districts largely altered in the redistricting process (usually those districts severely over- or underpopulated) with those that were not altered at all, or were only slightly altered.

In an article published at approximately the same time as Tufte's (Noragon, 1973), I employed two research strategies in a study of 32 states and 333 districts in an effort to discover what, if any, trace influences could be detected as a result of redistricting in the 1960s. First, through linear regression I sought to identify those districts most politically (read "competitively") affected through mapping changes from among "recognizable" districts, i.e., those occupying much of the same land mass after redistricting as they did before. I designated 35 districts that had been severely affected. The competitive nature of those districts, based on an adaption of Lewis Froman's index of party competitiveness, both before and after redistricting (through 1970) is instructive:

	Before Redistricting	After Redistricting
Very competitive or competitive districts	66%	29%
Semi-competitive or safe districts	34	71
	100%	100%
N = 35		

While almost two-thirds of these districts were competitive before redistricting, less than 30 percent were competitive afterwards—a rather dramatic reversal. Six "nonrecognizable" districts accrued from the redistricting process (new districts created from largely overpopulated districts). Of these six, four could be termed "safe" on the basis of subsequent election results.

My second strategy was to sort the 333 districts included in the study (minus five at-large seats) into two groups: those that were most malapportioned prior to redistricting, i.e., whose populations varied ±15 percent or more from the state norm; and the remainder, which were fairly well apportioned before redistricting. The first group of districts, numbering 158, were those most often, and most radically, carved by the legislative scalpel, and thus constituted the most likely place to seek the effects of redistricting. The competitive makeup of the two groups before and after redistricting is as follows:

	Malapportioned Districts		
	Before	After	Difference
Very competitive or competitive districts	46%	27%	−19%
Semi-competitive or safe districts	54	73	+19
	100%	100%	
N = 158			

	Well-Apportioned Districts		
	Before	After	Difference
Very competitive or competitive districts	37%	31%	− 6%
Semi-competitive or safe districts	63	69	+ 6
	100%	100%	
N = 170			

A comparison of the two groups shows that, although a decline in the proportion of competitive districts is evident in both (with an overall reduction of 11.6 percent), the decline in the more malapportioned subset is much greater: three times as many districts fell from the competitive categories in the malapportioned subset than in the "well-apportioned" subset.

To me, this evidence points to the independent influence of redistricting as a factor helping to account for the decline in competition in House districts, and suggests that Ferejohn was incorrect in dismissing Tufte's argument.

JACK L. NORAGON

Ohio Legislative Service Commission

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Noragon, Jack L. (1973). "Redistricting, Political Outcomes, and Gerrymandering in the 1960s." In L. Papayanopoulos (ed.), *Democratic Representation and Apportionment: Quantitative Methods, Measures, and Criteria. Annals of the New York Academy of Sciences* 219: 314–34.

Tufte, Edward (1973). "The Relationship between Seats and Votes in Two-Party Systems." *American Political Science Review* 67:540–54.

Reply

TO THE EDITOR:

I am somewhat confused by Jack Noragon's comment on my article, which appeared in this *Review* in March 1977. There I concluded that, since the level of competition for House seats declined in those states that had not been redistricted as well as in those that had, the explanation for this decline probably did not reside in redistricting. Noragon presents two tables apparently intended to call this conclusion into question. First, he shows that in the 35 districts he designates as "severely affected," the level of competition declined after redistricting. Second, he shows that the drop in competition following redistricting was larger in the severely malapportioned districts than in those which were relatively well apportioned prior to redistricting.

Do the data in either of these tables affect the conclusion presented in my article? The data in Noragon's first table are entirely consistent with my argument, while those presented in the second table suggest merely that among the seats that were redistricted there is some systematic variation in the effects of the redistricting. Although there may in fact be such systematic variation, this is irrelevant to the claim advanced in my article: the decline in competition occurs in those districts *that were not redistricted* as well as in the others. Thus, while some of the data presented in Noragon's comment are interesting, I fail to understand in what way they warrant a modification of my conclusions.

I suspect that the questions of interest to Noragon, especially in his earlier article (1973), are somewhat distinct from mine and that this fact has caused some confusion. In his first article he was interested in seeing whether redistricting had an effect, first, on the competitiveness of House seats and, second, on the partisan distribution of these seats. Third, he investigated various factors that might alter the effects of redistricting.

To investigate these issues, Noragon examined the competitiveness of districts before and after redistricting without using a control group of unredistricted districts. As should be clear, I object to the use of this method when the natural competing hypothesis is that all districts underwent changes at the same time. But perhaps this method retains some validity if one is interested in studying the effects of various factors on the impact of redistricting–Noragon's final set of questions.

Noragon implies in his comment and in his earlier article that two variables had a measurable effect on the impact of redistricting on the level of competition for House seats: the "degree of change in the district" (which might be measured by the malapportionment variable in the second table in his comment), and the partisan conditions surrounding the redistricting process. I believe the data which he reports in his original article do not support firm conclusions on these points and that, more likely, the general decline in the level of competitiveness dwarfed the impact of the other variables that Noragon chose to study.

The following table compares the effect of redistricting on those districts which were, by Noragon's criteria, "politically affected" by redistricting. Those districts were divided into two groups according to how much change occurred in their boundaries; he labeled these districts as "recognizable," or "nonrecognizable" (Noragon, 1973, Tables 3 and 4). Table 1 reports the change in safeness before and after redistricting in the politically affected districts. The data suggest, to me anyway, that, if anything, the effects of redistricting may be contrary to those suggested by Noragon. That is, among the districts "politically affected" by redistricting those districts which were substantially changed by redistricting exhibited an increase in the proportion of safe seats which was small compared to the corresponding increase in safeness in the districts less altered. Of course, the number of cases in this comparison is quite small and one would be unwise to conclude too much from the table. I would think a more prudent course would be to regard the impact of "degree of change in the district" on the decline of competition as ambiguous.

Perhaps more interesting is Noragon's classification of districts according to whether the redistricting had occurred in states in which the

Table 1. The Effects of Redistricting on Recognizable versus Nonrecognizable Districts*

	Before Redistricting	After Redistricting	Number of Cases
Recognizable Districts	.20	.57	35
Nonrecognizable Districts	.46	.50	26

*Entries are the proportion of safe districts.

legislature and executive were controlled (1) by a cohesive majority party; (2) those states in which two fairly cohesive parties divided control over the branches of government; and (3) those states in which redistricting took place under conditions in which the parties were relatively weak or in the one-party states. Noragon argues that if a cohesive majority party controls the executive and legislature it will try to increase the size of its congressional delegation by utilizing its supporters more efficiently, and thereby diminish the proportion of safe seats. In either of the other cases, the effect of redistricting should, according to Noragon, increase the proportion of safe seats.

The data presented by Noragon only permit us to observe the percentage of districts that moved out of the marginal category following redistricting. In the states which exhibited control of the executive and legislative branches by a cohesive majority party, there was a net change of 11 seats (of 69 total seats) or 16 percent out of the marginal cagegory. In the other states, a net total of 17 of 183 seats, or 9 percent, switched out of the marginal category. In other words, the decline in competition was, if anything, more pronounced in the states whose legislature and executive were controlled by a cohesive majority party than in the other states.

This result is, it seems, inexplicable by Noragon's own theory and should, I think, cause him to suspect that the phenomenon of the decline of competition for House seats is indeed a general one, and that it had little to do either with whether or not a seat was redistricted or with the conditions under which the redistricting, if any, took place.

JOHN FEREJOHN

California Institute of Technology

Reference: Noragon, Jack (1973). "Redistricting, Political Outcomes, and Gerrymandering in the 1960s." In L. Papayanopoulos (ed.), *Democratic Representation and Apportionment: Quantitative Methods, Measures, and Criteria. Annals of the New York Academy of Sciences* 219:314–34.

ON BOOK REVIEWS

Comment on Kahan's Review (Vol. 72, December 1978, pp. 1444–45)

TO THE EDITOR:

The most serious fault with Michael Kahan's (1978) review of my book *Power and Ritual in the Israel Labor Party: A Study in Political Anthropology* is his obvious failure to understand the main conceptual framework which informs the analysis of the work. For example, Kahan states that "most public or quasi-public political gatherings are, by nature, ritualistic; and Aronoff gathered his information almost entirely from such party gatherings." In fact, I devote significant parts of my theoretical introduction and the chapter dealing specifically with ritual in the Labor Party to making a careful analytic distinction between the imprecise everyday usage of the term "ritual" as used by non-specialists (exemplified by the above quotation from Kahan's review) and the more precise anthropological concept which I refine, adapt, and apply in my analysis. I clearly distinguish the unique ritual aspects of the highly secretive closed forum of the Standing Committee (which I was the first and only "outsider" ever allowed to observe), which was characterized by the controlled and bracketed social setting in which it took place, and by the fact that the outcomes were determined in advance. (Cf. p. 75 for an elaboration of the ten major characteristics of ritual in this forum.) I stressed the difference between the ritual character of the Standing Committee and the ceremonial aspects of more open forums such as the national party conference, which lacked important characteristics essential to ritual. Kahan completely ignored and/or failed to understand such important analytic distinctions, which are the basis of the conceptual framework.

Rather than criticize the appropriateness of the conceptual framework I employed, Kahan criticizes me for having failed to employ a more "dialectic" and "metaphysical" approach, which he evidently would have preferred. Yet, by failing to deal with the conceptual analysis I employed and by pushing his own (neo-Marxist?) approach, he distorts and misrepresents my findings. He falsely accuses me of having failed to appreciate the degree to which Labor has shaped Israeli society, and of having treated the party as a "surrogate for the culture" (whatever that means). That this is untrue is indicated in one of my concluding statements: "However, it is clear that in a political system in which the dominant political party has had such a great role in the shaping of the way in which the society has developed, the policies, procedures, and example set by the dominant party and its leaders have been an important factor in shaping and effecting as well as reflecting and articulating the values of the society as a whole" (pp. 176–77). Kahan failed to understand that my analysis of ritual and other forms of symbolic action were vehicles for focusing on the cultural dimension as it interacts with the major sociopolitical processes which shaped the development of Israeli society and the Labor party. In fact, I did stress the complexity of the mutual influence of social factors on the party, and of the party on the society.

Among the broad fundamental contradictions in Israeli society which Kahan falsely accuses me of having either ignored or ineffectively treated, he stresses the relationship between religion and the state. I quote from my conclusions to a lengthy analysis of the subject: "The *inherent contradictions* between the values of religiosly orthodox and secularly liberal ideologies about the very nature of the society and the state are less amenable to 'solution,' even by any expenditure" (p. 93, emphasis added).

Because I hope to have this reply published, I will cite only one more case among the many inaccurate accusations made by Kahan: his claim that I did not go beyond the conclusions of Medding's *Mapai in Israel* (Cambridge University Press, 1972). In fact, I spent a considerable part of chapter 6 and my concluding chapter analyzing why Medding and I came to fundamentally opposite conclusions about the role of democracy in the party. Medding concluded that Mapai was almost a classic case of a pluralistic democratic party which disproved Michel's "iron rule of oligarchy"; I concluded (on this point) that it was in fact very close to Michel's model. Joshua Sinai wrote in the Fall, 1977 *Middle East Review* (10:71): "Dr. Medding's assumptions about the party have recently been challenged by M. J. Aronoff's *Power and Ritual in the Israel Labor Party*. . . . Both books are necessary reading for anyone interested in the organization of Mapai/Labor Party, but Dr. Aronoff's study is the more penetrating." Regardless of whose conclusions one may find the more convincing, it is impossible to understand how any unbiased and conscientious scholar who had read our two books could have made a charge like Kahan's. I leave it to the objective readers to draw their own conclusions.

MYRON J. ARONOFF

Rutgers University

Comment on Freedman's Review (Vol. 72, December 1978, pp. 1531–32)

TO THE EDITOR:

When Robert O. Freedman wrote his first review of my book, *Red Star on the Nile: The Soviet-Egyptian Influence Relationship Since the June War*, for the *Middle East Review* (Spring 1977), I directed myself to his distortions, inaccuracies, and confused line of analysis in a letter to the editor (*MER*, Summer 1977). I shall not burden readers of these columns with a repetition of those remarks, though they are as germane now as they were then, since his second review (*APSR*, December 1978) is, with the exception of a few phrases and sentences, deleted possibly for reasons of space, the same one that he published earlier.

The submission of the same review to two scholarly journals raises fundamental questions concerning Freedman's professional judgment that say more about the nature of the review than any detailed comments I could provide.

ALVIN Z. RUBINSTEIN

University of Pennsylvania

Reply

TO THE EDITOR:

Alvin Rubinstein's ad-hominem attacks aside, here are the facts in the matter. When I was asked to review Rubinstein's book by the *APSR*, I wrote back informing the journal that I had already reviewed the book for the *Middle East Review*, a small journal dealing with contemporary Middle Eastern affairs. I indicated, however, that I would be happy to review the book for the *APSR*, given its different readership, if the *APSR* wished me to do so. The response from the *APSR* was in the affirmative and I prepared the review accordingly.

My approach to preparing book reviews is straightforward. In looking at the audience to which it is addressed, I seek to evaluate the book in question with a view toward helping the reader make a decision as to whether or not he or she should read the book, and/or order it for his or her college (or personal) library. In the book review which I wrote for the *Middle East Review*, which totaled some 1800 words—three times the length of the *APSR* review—I dealt in detail with a number of areas of particular interest for Middle Eastern specialists, such as Rubinstein's analysis of the Israeli response to Nasser's "war of attrition," Rubinstein's evaluation of the abortive coup d'état in the Sudan in 1971, and his chronological treatment of Sadat's preparation for war in the 1972–73 period. By contrast, my remarks in the *APSR* review were restricted to areas I thought would be of general interest to political scientists, regardless of their specialty within the discipline.

Rubinstein might have done a better service to *APSR* readers if he had responded to my substantive criticisms of his book instead of engaging in attempts at character defamation.

ROBERT O. FREEDMAN

Baltimore Hebrew College

Editor's Note

The Rubinstein-Freedman exchange requires editorial clarification. When we asked him to review the Rubinstein book in February, 1977, Professor Freedman replied that he had reviewed it already for the *Middle East Review,* but would like to "review it more extensively" for the *APSR*. Our normal policy is to seek another reviewer where our first choice is reviewing the book for another journal. Because I assumed that the *MER* review was to be short and felt that Freedman was the appropriate reviewer for this particular book, I made an exception to this policy. Of course, I expected quite a different review–covering some of the same points perhaps but certainly in different language and, as Freedman had offered, more extensive.

A comparison of Freedman's two reviews shows that my expectations were unfulfilled. The only thing that prevents the two reviews from being identical in all important respects is that the *MER* review is longer. Apparently Freedman simply cut words out of the original review to produce the *APSR* version. Only 68 words (out of about 1150) in the *APSR* review were not in the earlier review, and our manuscript editor was responsible for changing some of them. Furthermore, in my judgment, there is little substantive difference between the two reviews.

No author should be exposed to the same review in two professional journals. For our part (however unintentional) in this affair, we owe Professor Rubinstein our deep apologies. Beyond that, we feel it necessary to reiterate the obvious: our reviewers are expected to submit original work to us. We cannot check reviews to determine their originality, but have to depend instead upon the professional responsibility of our reviewers.

PAUL ALLEN BECK

Book Review Editor

Comment on Liu's Review
(Vol. 72, December 1978, pp. 1443–44)

TO THE EDITOR:

It would be a pity if Byung Joon Ahn's fine book (*Chinese Politics and the Cultural Revolution*) were deprived of the wide readership it deserves as a result of Alan P. L. Liu's unaccountably critical assessment. That review notwithstanding, Ahn's book is quite simply among the best two or three books available dealing with the policy conflicts of the late 1950s and mid-1960s in China. The book is based on thorough and meticulous research in all available documentary and emigre sources and must be considered the most basic and comprehensive account of that period. Not only does the book clarify our understanding of the substantive issues dividing the Chinese leadership, but it also offers a probing and convincing analysis of how decisions are reached in the bureaucratic arena of Chinese politics. It may be true, as your reviewer points out, that many of Ahn's conclusions have now become part of scholarly conventional wisdom about this era. But credit should be given where it is due. It is Ahn's fine scholarship on this period that has contributed most substantially to shaping that current scholarly consensus.

VICTOR C. FALKENHEIM

University of Toronto

Comment on Rich's Review
(Vol. 72, December 1978, pp. 1421–23)

TO THE EDITOR:

A recent review faults Robert L. Lineberry for using San Antonio as the site for testing hypotheses about inter-neighborhood service differences, on the grounds that San Antonio is a very poor site for learning about *metropolitan* service distributions. Rich (1978) argues that the arena of competition for life style advantages is now the metropolitan area rather than the central city, and in most large urban areas locational strategies for acquiring desirable combinations are pursued as often among jurisdictions as within them.

If the question is how to make the distribution of urban services more equitable for citizens of all income groups within a metropolitan area, it is possible that San Antonio may be a very good site for testing hypotheses about service distribution because such a large proportion of the SMSA population lives within a single urban jurisdiction. Thus, Lineberry's site may be representative of the single jurisdiction metropolitan area, which seems to be the goal of many reformers.

San Antonio for decades has ranked among the top two or three metropolitan areas in the United States in the proportion of both total and urban SMSA population within the central city. In 1970 it ranked third among all large (over 500,000 population) metropolitan areas in the percentage of SMSA population living within the central city (76 percent) and percentage of urbanized area population living within the central city (85 percent). As the result of some major annexations in the 1970s, these percentages are even higher in 1979.

Thus, San Antonio seems to be a good approximation of the prototype of a metropolitan area with a high proportion of the population within a single urban jurisdiction. This proportion is attainable for most of the metropolitan areas of the United States. Outside of New England, about 87 percent of the central cities of the 264 SMSAs recognized by the Department of Commerce at the end of 1972 have more than three-fourths of their city boundary adjacent to unincorporated territory, and less than 15 percent of the population of the SMSA is within suburban municipalities whose boundaries adjoin the central city. So, for most metropolitan areas in the U.S. it is still possible to develop the central city into a jurisdiction containing a very high proportion of the SMSA population, thus avoiding the jurisdictional problems allegedly hampering equitable distribution of urban services to all citizens.

This solution is, of course, not attainable for many of the largest SMSAs because they already contain a central city surrounded by large numbers of suburban municipalities. But this is no reason to fault Lineberry's choice of location to test his methodology, which can be applied in the majority of SMSAs.

If future applications of the methodology in such cities as Houston, San Diego, and Jacksonville shows results similar to those found in San Antonio, the case for a single urban government for a metropolitan area will be strengthened. If the distribution of services in other large cities is as equitable as was the case with San Antonio, we can place more confidence in the view that decentralization is undesirable and that further centralization of urban governments may be desirable.

C. J. HEIN

University of Missouri, Kansas City

Comment on Miller's Review (Vol. 72, September 1978, pp. 1029–30)

TO THE EDITOR:

The review by David Miller (1978) of *Justice, Human Nature, and Political Obligation* and *Alienation and Identification,* although intelligent and subjectively fair, contains major inaccuracies in the presentation of my positions, and in particular grossly misrepresents what Miller agrees is a key element in my argument concerning the objectivity of values.

Because of space requirements, I shall merely allude to, rather than discuss, Miller's awkward position on objectivity—which he seems to believe requires agreement among observers—and which I believe a careful reading of my discussion of objectivity in *Justice* could have clarified for him. Miller seems to denigrate hypothetical counterfactual analyses. Yet, for instance, the assertion that Pasteur was objectively correct in his "germ" theory and the French Academy wrong is not dependent upon agreement or its absence either then or now, but upon an assessment of the demerits of the academy's treatment of evidence and its understanding of the conditions of proof. Nor can it be divorced from hypothetical considerations. As Hempel has shown so convincingly, explanations (and, I would add, assessments) are acceptable only if they can sustain counterfactual subjunctive conditional tests. There are no simply "factual" elements in nature. My discussion of Toulmin in the area of epistemology was merely one example of how the methodology of employing counterfactual conditionals clarifies the reifications of common language.

Let us now turn from this preliminary clarification to the point of my letter, Miller's serious distortion of a central element in my defense of the objectivity of values: "[Kaplan's] 'test in principle' presupposes that moral disagreements arise only from imperfect information and/or personal bias on the part of the people making the judgments. It seems likely that there are value differences that cannot be accounted for in this way." I think it is unlikely that it is accidental, despite my clear and repeated statements to the contrary, that Miller attributes this absurdly simplistic position to me. If his position on objectivity reflects, as it seems to, a position that objective statements require a strong, or even unique, order in the world, then he likely jumped to the conclusion that I must be defending that position in the realm of values.

Perhaps Miller is not sufficiently aware that the concept of a critical experiment—which is intimately related to the concept of a strong order—has been discredited. Many scientific "truths" are so strongly established that they seem to imply a strong order everywhere in the world. However, it is a serious mistake to conflate a few very important areas of science with all of science or logically derived theorems with all of scientific reasoning. For instance, there may be competing but objectively weak solutions to some problems; e.g., whether dinosaurs were ectotherms or endotherms, whether a crystal is living, the implications of the red shift in astrophysics, the first-order disagreement and second-order (symmetrical) agreement concerning which system is moving with respect to which between observers on different inertial systems, the optimal way to design an air-warning system (optimal systems being distinguishable from non-optimal systems even when there are more than one of the former), and so forth. In some cases more information would produce a unique answer, whether with respect to a proposition or competing theories. In other cases, common first- but not second-order symmetrical agreement is ruled out. In some cases, there may be solutions that objectively are logically or empirically equivalent and not in other cases. Yet all these positions are objective.

Note how my argument on values fits into this "continuum." Apart from the fact that I sharply distinguish between first-order value judgments, where I deny the probability of consonance, and second-order moral judgments, where I assert its plausibility, the following points are relevant and are clearly stated. On p. 92 of *Justice,* I state: "Objectivity in the only sense in which it is meaningful, is possible in that realm [of values] also. . . ." I do not say that it is omnipresent, but only that it is possible. On p. 100, I point out that judgments concerning the comparative value of different societies are "weakly ordered" and that "because our judgments concerning values are often low-confidence judgments, they come closer to what the *ancients* [italics added] called opinion than to [what they called] science." On p. 101, I analogize that because a "color-blind person cannot distinguish some colors . . . does not mean that he can distinguish none." Apart from the fact that it is not clear whether Miller is talking about first- or second-order value differences—a distinction in the absence of which one cannot begin to understand my position—in the realm of values (and in the realm of colors as well) there are neighboring positions that may be indistinguishable or equivalent, although in the realm of values the "scale" is very complex; and differences concerning comparative evaluations of entire value systems and of alternative values within particular systems require different treatment. On p. 103, I ask whether there might be substantial biological differences among individuals such that they have no common second-order framework of values or whether there may be a test in principle for a best physiological type, in which case there would be a common second-order standard of values. Furthermore, I refer (p. 102) to possible areas of evaluation that are "personal or idiosyncratic," and thus that in principle are not objective. (I nowhere call this a "bias"; it would be an irreducible difference.)

This use of objectivity, which I believe conforms better to the actual praxis of science than Miller's, makes objectivity meaningful in the analysis of values. The extent to which human beings inhabit a common (or symmetrical) second-order moral universe is, however, open to inquiry.

There is no space to cover other important topics. Therefore, I can say only that Miller's other critiques are somewhat less than obviously relevant.

MORTON A. KAPLAN

University of Chicago

Reply

TO THE EDITOR:

Reviewers are necessarily condensers and simplifiers of the material they discuss, and Morton Kaplan should not complain if I have overlooked some of his qualifications and retractions in order to present his main line of argument as forcefully as possible. Many people will be interested in a defense of the view that value judgments are objective; rather fewer will be riveted by the thesis that perhaps they are objective and perhaps not. Yet to be absolutely fair to Kaplan, this second thesis is the one I should have to attribute to him.

Distortion, however, is another matter. Kaplan charges me with having simplified his argument for the objectivity of values to the point of absurdity, and with having neglected essential distinctions such as that between first- and second-order value judgments. I believe that the argument is basically defective and cannot be salvaged by any such devices. My reasons follow.

The central issue between us is whether there is sufficient similarity between empirical and moral judgments for the latter to be described as "objective." I hold that objectivity should be predicated on classes of judgment and that a necessary condition of the predication is *practical agreement among those making* judgments of a particular kind. A judgment such as "The boiling point of alcohol is lower than that of water" can be classed as objective because observers concur in it, or if they do not, they can agree on a procedure for resolving their difference. A judgment such as "Chocolate ice cream is nicer than vanilla" must be classed as subjective, on the other hand, since it neither commands agreement in practice nor is there any way forward for those who disagree.

I do not infer from this, as Kaplan supposes, that the truth of scientific theories depends upon the consensus of opinion among scientists. The connection between scientific theory and empirical evidence is sufficiently indirect to make it an open question at any time which of two competing theories offers the better explanation of a body of evidence. If science as a whole is to be called objective, however, it must be because its theories are ultimately verifiable by reference to a set of agreed-upon empirical judgments, namely, those of common-sense observation. Although Pasteur and the French Academy may have differed over the germ theory of disease, they agreed at least about which patients had the measles, and thus provided the evidence against which the theory could eventually be tested.

The question is whether we can find an analogous foundation for moral judgments. Kaplan appears to concede that there is unlikely to be agreement in what he calls "first-order" judgments—presumably the judgments we make as we go about our everyday business. He thinks agreement may be possible in second-order judgments, made under the conditions of the "test in principle." This involves thinking ourselves empathetically into the various roles offered by different social systems, and then making comparative judgments about the overall goodness of each system. How will this alter our first-order judgments? As far as I can see, it eliminates the effects of personal interest and of restricted information, but nothing more. If my favorable assessment of system *A* is governed by my privileged position in *A* and/or my ignorance of the satisfactions offered by alternative system *B*, then undergoing the test may lead me to revise my judgment. But suppose I am a dedicated and well-informed egalitarian who ranks systems solely on the basis of their degrees of equality; then the test will change nothing.

Kaplan suggests that we may at least be able to agree on a weak ordering of systems using the test in principle. Supposing this is so, how does it bear on the objectivity of our judgments? Compare the following two cases: (1) A group is asked to place very similar pieces of wood *A . . . . Z* in order of length; they find they can agree that *A* is longer than *W*, *P* than *D*, etc., but cannot give a complete order. (2) A group is asked to rank varieties of ice crean *A . . . . Z* in order of taste; they find they can agree that *A* is nicer than *W*, *P* than *D*, etc., but cannot give a complete order. Despite the formal similarity of the two cases, their implications are very different. In the first we have judgments that are objective but incomplete; the incompleteness is due to limited powers of sensory discrimination and could be remedied, e.g., by supplying the group with a micrometer. In the second case, the judgments made are subjective but partially convergent. There is no way of converting the weak ordering to a strong ordering, and no guarantee that an individual joining the group wouldn't weaken the order still further by preferring *W* to *A*. Now which kind of weakness is involved when moral judgments about the relative worth of social systems are made? In my view, clearly the second. Moral judgments differ because each person uses a slightly different combination of criteria in making them. Since, however, the number of criteria is in practice finite, it seems likely that we shall be able to describe hypothetical societies *p* and *q*, such that in everyone's book *p* is preferable to *q*. But there is no way in which this can be extended to give a complete ordering of all actual or possible societies; there are no moral micrometers.

Mention of criteria for moral judgments shows that the analogy with judgments about ice cream is not complete. Reasons can be given for our moral assessments and to the extent that others share these reasons, moral argument may be possible. If Kaplan would like to say that this constitutes a degree of objectivity in moral judgment—and he extends an olive branch with his mention of a "continuum" (I presume between objectivity and subjectivity)—I am happy to accept the compromise. But if lines have to be drawn, I must reaffirm my conviction that such judgments lack the basis in agreement that makes it possible for us to describe empirical judgments as objective, and thus fall much closer to avowedly subjective judgments of taste. The problem about moral judgments is not merely that they are complex, as Kaplan suggests, but that there are irreducible variations in the criteria used to make them.

DAVID MILLER

University of East Anglia

EDITORIAL NOTE

Paul Allen Beck is leaving the University of Pittsburgh to assume a post at Florida State University. He has served as Book Review Editor since 1976. Professor Beck's service to the Association in this important position is best exemplified by the resolution of commendation passed by the APSA Council:

> The APSA Council hereby commends Paul Allen Beck for his superlative service to the Association as Book Review Editor, 1976–1979. Professor Beck's management of this important section of the journal, his judicious selection of books and reviewers, and his advice and counsel in policy matters merit the gratitude of the entire membership.

At one time it was simple enough to manage the book review section from a different location. The sheer volume of work associated with this position now makes it imperative that the book reviews be assigned and coordinated from the journal's main office. I am pleased to announce that Holbert N. Carroll has accepted the appointment of Book Review Editor, effective July 1, 1979. Professor Carroll has served on the faculty of the University of Pittsburgh since 1946, chairing the department, 1960–1968. A person with wide interests in political science, Professor Carroll has written works on Congress and American foreign and defense policy. He served on the APSA Council 1967–1969. The Association will be well served by this acknowledged scholar and administrator.

* * * * *

With the backlog issue resolved, it has been possible to focus on other matters associated with managing the *Review*. One of an editor's responsibilities is to point to areas which researchers have neglected and to encourage scholars to venture into new directions. Despite the considerable interest shown by political scientists in public policy research during the last decade, relatively few such manuscripts are submitted to the *APSR*. I recently wrote to a number of policy scholars about this situation and several reasons for it were offered: e.g., not all that much quality work is being done, such research is often supported by government grants and published in reports, many other publishing outlets have recently been developed, and the *APSR* is perceived as not being hospitable to policy research. As editor, I can do little about the first three, but find it unfortunate if the last reason applies. And yet I can understand how such an attitude might develop, given the scope and variety of what is called "policy research." A useful purpose can be served, therefore, by identifying the type of policy-related study seemingly appropriate for the *APSR*. I prepared the following statement (now slightly revised) for a *Policy Network Directory*, to be published by the Policy Studies Organization. It is offered here to provoke thought, not to establish an orthodoxy.

> Articles directed to conceptual and theoretical development and refinement would certainly be of interest to our general readership. Comparative analyses of public policies, major issues, political institutions, policy processes, etc., would also be welcome because they provide the empirical basis for theory and conceptual growth. And single-issue studies too, might be of general interest if designed to test propositions drawn from the work of other policy scholars. Strictly substantive policy analysis—i.e., work designed to evaluate the substantive effects of existing programs and/or

> recommend programmatic change—is probably best placed in the several journals now inviting such work.

I also sent this statement to a number of policy scholars for their reaction. For the most part their reaction was positive. Several had constructive comments and as a result I made some revisions in the statement. Two responses were particularly contributory and deserve special mention here. Duncan MacRae, Jr., University of North Carolina, noted that: "There is one type of 'policy analysis' that does ... belong clearly in political science and this is the choice among political institutions—constitutions, federalism, party systems, electoral systems, administrative structure, legislative institutions, judicial systems, and the like." He also observed that while articles dealing with conceptual and theoretical development are relevant, "It might be useful to specify some of the major lines of development. . . ." Here is what he suggests:

> 1. Criteria for policy analysis [are needed]. This topic overlaps somewhat with political philosophy, especially when choices of regimes or constitutions are involved. But the region of overlap of criteria, between constitutional choice and program choice, seems to me to be among the most interesting areas: how can similar criteria be formulated for these large choices and for choices among specific programs? Can detailed choices among political institutions be assessed in economic or utilitarian terms? Can criteria, dealing with human development or perfection, be expressed in researchable terms?
>
> 2. The role of policy analysts in political systems is another large topic that deserves discussion. It can be generalized in one respect to deal with the proper "politics of expertise," and in another to deal with the evaluation of proposed new institutions, such as "science courts," to make use of policy-relevant expertise. An increasing variety of such institutions is being proposed and requires systematic analysis.
>
> 3. Normative analyses of how problems are formulated might be appropriate. Political scientists have contributed a great deal to the study of how issues *are* redefined; but could we take the further step of analyzing how issues *should* be redefined? At first glance, this seems to be mere assertion of the writer's political preferences; but if redefinition could derive from general philosophical systems, take a long view, or contribute to policy analysis, conceivably it could be a contribution to the *APSR*. I suspect that this sort of discussion has occurred more often in "great debates" on foreign policy or international relations than in domestic policy; but I wonder whether this restriction is necessary.
>
> 4. Can comparative policy studies be placed in a context of explicit evaluation of alternative choices currently facing a single polity? I have found "comparative public policy" to be a very frustrating field on the whole because of its lack of direct contribution to policy evaluation and choice. Explicit attention to policy choices, rather than to non-manipulatable variables alone, might help to remedy this.
>
> 5. Careful analyses of "political feasibility" are still needed. I don't mean mere case studies of feasibility; important as they are, I don't think the *APSR* should invite them. Rather, I mean analyses of the scientific versus intuitive or personal status of this topic. Not only the scientific status of feasibility assessment, but also its ethical status should be assessed. Should political science be training feasibility experts as "hired guns?"

This is a particularly rich and stimulating, though by no means comprehensive, list of research issues and is reprinted here for illustrative purposes. It encourages the political scientist to move beyond tightly formulated empirical work to consider its significance in the world of policy choice. It asks as well that scholars draw upon their training in philosophy for identifying the nature and consequence of decisions.

Theodore J. Lowi, Cornell University, asks why the criteria in the statement should not also be applied to other research areas. He writes:

> I would urge you to take it one step further and to formulate some of the same criteria on a more general basis for every area in which political scientists write articles and submit them for publication in the *APSR* which were perfectly adequate and acceptable from the standpoint of the subdiscipline in question, but which were too specialized and too narrowly focused to be in the *APSR*. If you had applied the criteria suggested in your September 12 memo to those areas, many such articles would have been sent back to the author with strong encouragement to divert them to the more specialized journals. Why, for example, should the *APSR* be more interested in an article on voting behavior in Congress or in the electorate if it is not "directed to conceptual and theoretical development and refinement?" Why should the *APSR* be receptive to an article on any subject if the article does not "provide the empirical basis for theory and conceptual growth?" You are correct in your position toward public policy papers, and you would be precisely equally correct in applying such criteria to all other areas of our discipline.

Whether a single statement is applicable to all subfields is not something I am prepared to judge at this point. I was moved to develop a statement for public policy for the reasons stated in my opening remarks. It is my belief, however, that the editor must become as knowledgeable as possible about developments in the many subfields of the discipline. Accordingly, it is my intention to correspond with a representative group of scholars in various fields to determine how they see the *APSR* serving them, as well as what they judge to be important developments in their particular corner of the discipline. In the meantime, I invite members to write to me or members of the editorial board on this important set of issues.

* * * * *

The following articles have tentatively been scheduled to appear in the December, 1979, issue:

Ruth Berins Collier and David Collier, University of California, "Inducements versus Constraints: Disaggregating 'Corporatism' "
Enrique A. Baloyra, University of North Carolina, "Criticism, Cynicism, and Political Evaluation: A Venezuelan Example"
Robert B. Albritton, Virginia Polytechnic Institute and State University, "Social Amelioration through Mass Insurgency? A Reexamination of the Piven and Cloward Thesis"
Frances Fox Piven, Boston University, and Richard A. Cloward, Columbia University, "Electoral Instability, Civil Disorder, and Relief Rises: A Reply to Albritton"
Dale C. Nelson, Fordham University, "Ethnicity and Socioeconomic Status as Sources of Participation: The Case of Ethnic-Political Culture"
W. Phillips Shively, University of Minnesota, "The Development of Party Identification among Adults: Exploration of a Functional Model"
Gregory B. Markus and Philip E. Converse, University of Michigan, "A Dynamic Simultaneous Equation Model of Electoral Choice"
Benjamin I. Page, University of Chicago, and Calvin C. Jones, National Opinion Research Center, "Reciprocal Effects of Policy Preferences, Party Loyalties and the Vote"
James H. Kuklinski, Indiana University, and John E. Stanga, Wichita State University, "Political Participation and Government Responsiveness: The Behavior of California Superior Courts"
Fred Thompson, Economic Council of Canada, "American Legislative Decision Making and the Size Principle"

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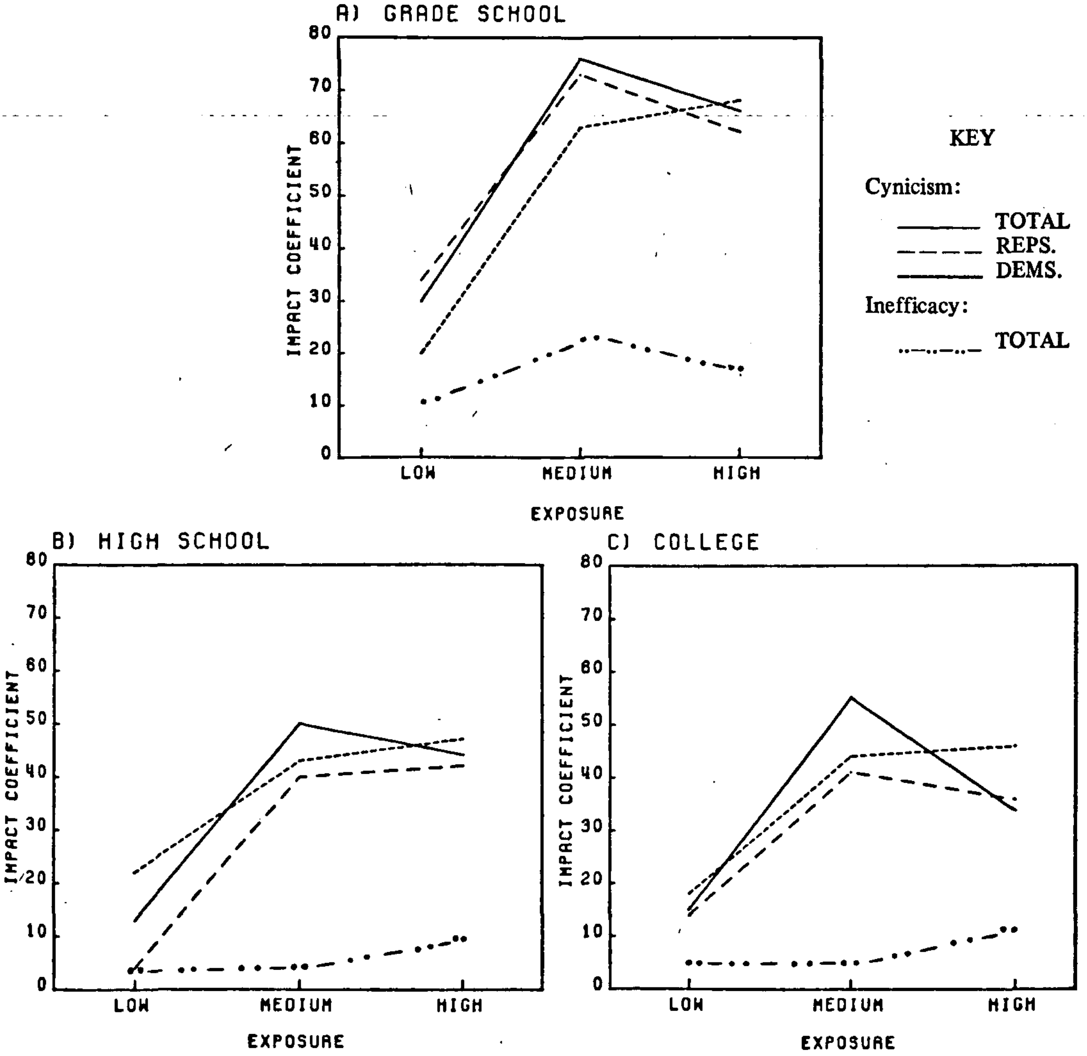
Erratum

The key was inadvertently omitted from Figure 3 of Arthur H. Miller, Edie N. Goldenberg, and Lutz Erbring, "Type-Set Politics: Impact of Newspapers on Public Confidence," Vol. 73 (March, 1979), p. 77. The figure, with key, is reprinted on p. 845.



Source: Center for Political Studies, National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a]The scale for the impact coefficients must be multiplied by .001 to equal the unstandardized regression coefficient. See Appendix B for the complete equations.

Figure 3. Interactive Impact of Newspaper Criticism and Exposure Level on Political Cynicism and Inefficacy[a]

BOOK REVIEWS

Political Theory and Methodology

Political Philosophy and the Issues of Politics. By Joseph Cropsey. (Chicago: University of Chicago Press, 1977. Pp. ix + 329. $22.00.)

This book collects 20 essays written for various occasions between 1955 and 1976. Yet, from beginning to end, there is a unifying perspective bespeaking a settled disposition. The disposition is constituted in "a lengthening experience with the texts [which] persuades me that, however consequential the gap between antiquity and modernity may be, it is not so radical as to have made philosophy itself impossible" (p. viii). That is, Joseph Cropsey finds a persistency in human experience about which it has been possible to think intelligently in every generation, and, further, the result of that reflection in each generation has not been inaccessible in succeeding ones. Consistently with this profession of confidence in political philosophy's constant implication in human experience, Cropsey provides an effective response to the repeated announcement of its demise demonstrating in each essay "benign, sober, incisive, and wise investigation" of the problems for which no age has found unproblematic solutions. To him, trying to dispense with philosophy will only intensify our difficulties to the extent we lose "that skepsis that looks beyond the horizon of modernity" (p. 154), leaving us unable to distinguish founded revulsion at aspects of modern life from unfounded (self-contradictory) efforts to revolt against "human life as men must live it."

The 6 essays comprising the fourth part of the book are particularly striking attempts to test the moderns and ancients against each other. Paying careful attention to the details of the works he discusses, Cropsey gives convincing evidence that, for example, Aristotle anticipated Machiavelli's exploration of mastery; and Hobbes did not so much abandon the thoughts of his great predecessors as revise them in such a way as to inaugurate that series of revisions which constitutes the story of modern philosophy's love-hate relation to what preceded it. An excellent elaboration of this ambiguity is provided in the author's recurrent comparisons of Rousseau's *Emile* to Adam Smith's *Wealth of Nations*.

But the subject of this book is also "issues of politics": conservatism, liberalism, radicalism, modernization, justifying force in the nuclear age, foreign aid, the fate of the family and property. What unites the discussion of each of these is Cropsey's desire to know whether the peculiar crisis of confidence in our time, reflected in policy making that is either guilty about, or disgusted with, "acquisitive egotism," is best diagnosed in terms of "alienated man" (selfish and claiming rights) seeking "identification" (collectivization in the experience of compassion or altruism); or in terms of "stunted man" whose "avid pursuit of wealth evinces a deficiency in the dignity, self-regard, or true self-sufficiency of the man who practices it as a way of life, the man who personifies the moral habit of treating small things as if they are important and is not ashamed to manifest his passion for things wanted. If the judgment of capitalism were undertaken from the standpoint of magnanimity rather than alienation, the verdict would be that acquisitive egotism is offensive to self-regard rather than to altruism" (p. 216).

Nor is there any doubt about Cropsey's inclination in the matter: "It seems to me that mere observation of our contemporary circumstances supports the judgment from the standpoint of magnanimity better than it supports the judgment from the standpoint of alienation" (p. 216). He distinguishes "competitiveness," which is spirited if not "noble," from "acquisitiveness" which is "more objectionable as a sign of deficient than of excessive self-esteem" (p. 216). He concludes: "On a moral question pertaining to the way of life where private ownership is the rule, the weight of presumption is against the less demanding solution of the question. By the less demanding I mean that solution which favors either the principle of individual preference or the supremacy of the antialienation criterion rather than a prescription favoring the moral fortitude of the individual or community" (p. 217).

This conclusion arises from the reflection that alienation is not first produced by competitiveness but by "otherness" without which no one could identify oneself (pp. 44–45). Identifying oneself through the experience of otherness leads to assessment of the various experiences of otherness and a discrimination between just and unjust distancings. The question will be, what is a just distance between one and another?—not, is it pleasurable to live in a world of "others?" Insofar as starting from "alienation" obscures the difference between these questions, it is either misleading because it holds out the hope that the just and the pleasant must converge, or useless because it symptomizes a problem without advancing our knowledge of the distinction between the just and the pleasant (p. 49). Needless to say, the inevitability of this issue, both in antiquity and modernity, is manifest in all important debates over both foreign and domestic policies.

In the end, the reader is impressed by the fact that Cropsey's modesty with respect to conclusions is amply compensated for by his elucidation of what is to be thought about, and by his quiet but unswerving refusal to compromise the injunction to go where the argument leads.

TIMOTHY FULLER

Colorado College

Political Language: Words That Succeed and Policies That Fail. By Murray Edelman. (New York: Academic Press, 1977. Pp. xxii + 164. $12.50.)

Murray Edelman continues in this new text to explore the ways in which public language is deployed to manipulate belief, evoke emotion, and shape conduct. A vocabulary which one constituency, typically a professional or political elite, applies to "client" constituencies, operates to reassure the latter or temper their grievances or translate their troubles into personal illnesses. To be unaware of the political uses of the vocabularies of science, medicine, education, law, and bureaucracy is to be treated, handled, controlled and confined in manipulative ways. The vocabularies Edelman studies generally depoliticize subordinate populations. Particularly when the vocabulary is based on the ideology of "helping" people with problems, the language, and the treatment it sanctions, implicitly legitimize the conditions which subordinate the populace.

This is an absorbing book. It is carried by its examples. But it also seriously flawed. Edelman refuses to provide theoretical support for this particular interpretation of our public language. He says, "I am concerned with how beliefs come to be evoked and with their consequences, not their tenability." But this won't do. Concern with the evocation and consequences of beliefs is also and necessarily a concern with their tenability. If a vocabulary is used to express tenable beliefs and the believers have good reasons for holding them, we do not need to explain further why people express those beliefs through that vocabulary. One example may be helpful here. Edelman (p. 60) says that the habit of labeling the activities which mental patients engage in as dance "therapy," recreation "therapy," etc., "treats a common activity as though it were a medical one"; it "superimposes a political relationship on a medical one" by legitimizing the subordinate status of those receiving the therapy. But this reading requires a prior conclusion that the participants are not correctly or sufficiently categorized through these medical terms, that their condition is one which does not require "therapeutic" relationships. For if such a relation is necessary, if it is legitimate, then there would be no necessary political relation hidden within the medical vocabulary. It is because Edelman believes or suspects that medical terms are often used to control a populace which is inadequately comprehended through them that he exposes the political relation inside the medical. But in this instance, as in others, he does not defend an interpretation to support this suspicion. It is merely insinuated into the expose. By exposing all vocabularies and endorsing none, Edelman implicitly endorses the cynical view that all uses of language are thoroughly manipulative. But he cannot accept that thesis thoroughly, or else it would undermine the credibility of his own thesis. To complete his project Edelman must endorse and defend a political interpretation which sanctions the characterization he has offered of professional vocabularies.

I think this is a significant flaw in an otherwise admirable volume. The admirable part is that Edelman has provided new illustrations, rich in detail, for those who would arm themselves against the language games prevailing in the professions and the polity. It is a valuable handbook, especially for those who seek to reappraise the role of the "helping" professions in American society today.

WILLIAM E. CONNOLLY

University of Massachusetts, Amherst

The Future of Technological Civilization. By Victor Ferkiss. (New York: George Braziller, 1974. Pp. 369. $12.50.)

In *Technological Man* (1969), Victor Ferkiss concluded that a new human type capable of using technology for humanistic purposes was not only desperately needed, but could be realized. In his new book, he undertakes a very ambitious, but welcome, effort to delineate the foundations for a political philosophy that he believes is needed for the construction of a social order based on a new world outlook that he terms "ecological humanism."

Ferkiss agrees with those that see mankind threatened. In general, he accepts that critique of American society which sees it permeated with alienation, mechanization, and bureaucratization.

The enemy against whom Ferkiss wields his theoretic lance is none other than Liberalism (chapters 1–6). The failure of liberalism lies in its ideology, the Lockean heritage of possessive individualism, and its rampant self-centered aggrandizement. Locke's teaching "bestowed on us a ravaged continent and a disordered society, and threatens us with the loss of our humanity in enslavement to technology" (p. 31). Modification or reform of liberalism, whether masquerading under the form of state capitalism, monopoly capitalism, or interest-group liberalism, will not work. Ferkiss sees little hope in conservatism and socialism, for the alternatives which they offer, he concludes, lead to similar dead ends.

Ferkiss' attack on liberalism provides an impressive synthesis of the increasingly large literature on this topic. Granted, much of his criticism is valid, yet the problems that he points to are common to all industrialized societies, east and west. A major part of our problems may stem from the fact that the very foundations of our modern, technological-scientific world order are intrinsically power-oriented. The drive for power and dominion over nature (and over other people)—part and parcel of the modern outlook—owes as much to Descartes and Bacon as it does to Locke and Hobbes. Ferkiss appears to be setting up a straw man, and in doing so he fails to come to grips with the inner logic of the technological society that has emerged. It is the dynamic, if not demonic qualities of parts of the technological society that Ferkiss will not confront. This is understandable, since he is such an outspoken believer in the positive attributes of technology.

"Ecological Humanism" (chapters 7–14) is the heart of Ferkiss' effort to lay the groundwork for a political philosophy for the future. He argues persuasively that political philosophy must become deliberately normative, universally relevant, and capable of being brought to bear on the emerging political and social realities in concrete ways. He makes a concerted effort to integrate knowledge of science and ethics, philosophy and politics, economics, history, and ecology—in addressing some of the basic questions revolving around the concepts of human nature, nurture, freedom and values. On the whole, this is the most original and strongest part of the book. Ferkiss takes a positive view of politics. His perception of human nature is basically optimistic, and essentially follows Rousseau. The good life and the good society are possible, and given the right value orientation, can be implemented (p. 127). Ferkiss rejects the argument that the concept of the public interest or the common good is relativistic and basically empty (chapter 13). He maintains that there is a substantive public interest and that its realization centers around the needs of subsistence, order, and purpose. Ferkiss considers the distinction between compulsion and consent arbitrary (p. 53) and maintains that the distinction between human beings as political and social animals is largely meaningless. Freedom is seen not as an end in itself but as a mode of activity, whose purpose is to increase the number of choices open to human beings and the ability to decide what those choices shall be. According to Ferkiss, the primary problem areas to which ecological humanism must address itself are those pertaining to the relationship between humanity and nature, on the one hand, and between humanity and the machines we have created, on the other. Noticeably missing is any overt attention to the category of problems concerning humanity's relationship to humanity.

Two chapters are given over to an explication of what Ferkiss calls "necessary utopia." Ferkiss will not specify the particular means and structures for accomplishing the goals that he specifies. Nor does he provide more than a set of rather general guidelines of what is needed: leadership, planning and political will, political intelligence, and political power. All this is discussed in so cursory and general a fashion that the conclusions give the appearance of being rather obvious, and even trite. Ferkiss' utopian vision lacks the concreteness and imagination that would make it more compelling. He depends upon a persistent and continuing series of hortatory admonitions of what "must" or "should" or "ought" to be done, which turns out to be a poor substitute for careful utopian thinking.

Two short chapters deal with how to bring about the new political order. In essence, Ferkiss puts his faith in a nonviolent "immanent revolution." Though he denies it, his appear for a changed consciousness comes at times very close to *The Greening of America.* Think peaceful revolution and you will enter that kingdom! Ferkiss sees the emergence of a new elite, or of great individuals, as the harbingers of the transformation of consciousness. He suggests that those who oppose ecological humanism are beatable (p. 163). As he puts it, "War, pollution and dehumanization could not exist unless it was in the interest of powerful individuals and groups that it be what it is. Obviously then if there is a door leading to escape the keys will have to be wrenched from the hands of humanity's jailers. This means that some sort of political action to free man from the megamachine will be necessary" (p. 79). But no real guidelines are provided to assist in this particular task.

Ferkiss believes that technology is completely controllable by human will and rationality. He fails to understand that every technology has unforeseen consequences. Ferkiss' assumption that "the future is completely open to human decision-making, whether for good or ill" (p. 7), reflects precisely the kind of hubris that has got us into the predicaments to which he addresses himself. His optimism at times strikes one as naive. The ecological humanistic revolution "will be an 'open conspiracy,' an unstructured, unorganized movement for reform" (p. 270). Shades of the progressive movement! The immanent revolution will take place because it must (i.e., ought to) take place. Given our knowledge of human history, such optimism really seems unfounded. We are offered a politics of hope, and in these gloomy days, that is most welcome. Nonetheless, one needs to be careful not to be ensnared by the siren song of misdirected optimism. We will need a more rigorous and radical critique to chart us through the stormy waters ahead.

JOSEPH HABERER

Purdue University

John Locke and the Theory of Sovereignty: Mixed Monarchy and the Right of Resistance in the Political Thought of the English Revolution. By Julian H. Franklin. (New York: Cambridge University Press, 1978. Pp. xi + 148. $15.95.)

If this volume were intended for physical rather than mental consumption, its labeling would surely be controversial; it is not primarily about John Locke's theory of sovereignty and resistance. Fewer than eight pages are predominantly devoted to explicating Locke's views on these topics (pp. 91–98). Although this non-comprehensive treatment of the subject is supplemented in the course of comparisons between others and Locke, the result remains an incomplete study of Locke's pertinent views. Indeed, the protagonist of the work is George Lawson, not John Locke. Ultimately, however, rather than being a study of any one theorist, this book is a study of the development of the concepts of sovereignty and resistance throughout the seventeenth-century Parliamentarian/Whig tradition. It focuses on the problem of resistance to sovereignty in a mixed constitution.

Franklin frames the problem as it was inherited from sixteenth-century constitutionalists. The monarchomach tradition advanced a right of resistance to monarchs who flagrantly violated the conditions upon which they received the consent of the community, the source of all legitimate authority. This popular constituent power, however, was viewed as resting in the governmental body by which the people were represented. Logically, though, such a locus of the constituent power is incompatible with mixed monarchy, for the king's sphere of independent power within such a government is thus made potentially subject to revocation by the Estates. Resistance is preserved, but mixed monarchy implicitly falls. Nevertheless, circumstances permitted ignoring this implication.

The English Civil War ended the possibility of such obscuration, since the king's opponents found themselves in the position of justifying a war against him while insisting on his independent authority. Parliamentary publicists Henry Parker, Charles Herle, William Prynne, William Bridge, Jeremiah Burrough and Philip Hutton are each shown to involve themselves in implicit or explicit contradictions, as they inevitably assert both parliamentary supremacy as constituent power on behalf of the people, and some version of independent royal power in a mixed monarchy. (I say "inevitably" because most, perhaps all, theories are presented as retrospective attempts to rationalize an already established set of goals. Indeed lacunae in positions are filled on this principle: "It is hard to believe, then, that Lawson would have held a principle that was of little value to his cause and universally repudiated by persons of his social rank" [p. 128].)

George Lawson solves the Parliamentarians' dilemma. The constituent power lies in the community. This entity is created by the consent of its members to form a government. The community retains its constituent power even after it has established a government. Thus resistance to royal tyranny by the community became compatible with royal independence within a mixed government.

What of Locke? "He had found the solution ready-made in Lawson. But he put it in a form and language that made it impossible for his own and later generations to ignore" (p. 123). Franklin's discussion of Locke on sovereignty and resistance is limited in scope and form to supporting the thesis that it was taken over from Lawson. Locke's treatment of the nature and limits of the right to disobey a bad—as opposed to a tyrannical—law is ignored (*A Letter on Toleration,* in the Tegg edition of *The Works of John Locke,* Vol. 6, pp. 43–44). Yet this, rather than his right of revolution, is the basis of his concept of civil disobedience.

Great stress is placed on the concept of the community in Locke, for this is essential to identifying his position with Lawson's. While this concept figures in Locke's explanation of the formation of government, the preponderance of his consideration of the dissolution of government suggests a return not to community but to the "loose state of nature" (*The Second Treatise,* in *Works,* Vol. 5, pp. 464, 465, 469–70, 473, 476). Franklin deals with this difficulty for his thesis only by referring to "two or three misleading phrases in Chapter XIX" while comparing William Atwood to Locke. Could Locke's thoughts on the matter be more complex than a mere borrowing from Lawson? The possibility deserves serious consideration.

The value of this work lies in its development of the Parliamentian position on sovereignty and resistance, especially that of George Lawson. It should be read for this. But no one should be deceived by its title into expectations of intensive Lockean scholarship.

FRANCIS EDWARD DEVINE

University of Southern Mississippi

Die Krise des Amerikanismus: Revolutionäre Ordnung und gesellschaftliches Selbstverstaendnis in der amerikanischen Republik. By Jüergen Gebhardt. (Stuttgart: Ernst Klett Verlag, 1976. Pp. 337. DM 68.)

Jüergen Gebhardt, a West German political theorist, has written a remarkable book about the American civil theology. Bringing his knowledge of the history of political philosophy to bear upon the problem of American political self-interpretation, Gebhardt has produced an original and sympathetic account of the American political style. Particularly refreshing is Gebhardt's appreciation of the intellectual and spiritual quality of the political thinking of the Founders. There is here none of the ignorant contempt for American ideas and institutions apparent in the writings of, say, Martin Heidegger. At the same time, Gebhardt's book reflects the philosophical depth we have come to associate with the best of German thought. Like de Tocqueville, Gebhardt helps us to see ourselves more clearly by virtue of the fact that from his cultural distance he can make out the extent to which Lockean contractualism permeates our thinking with at times seriously distorting effects. Gebhardt does not make the mistake of labeling Lockeanism as our only political theme, however. Like Robert Bellah he also stresses the importance of the classical republican and biblical motifs.

For Gebhardt, the American Revolution was inspired by a genuinely revolutionary consciousness, itself the result of a merger of classical republicanism and Christian millenarianism. At the same time, the Revolution did not derail into what Eric Voegelin has called metastatic fanaticism; it kept its balance through a strong admixture of "common sense" as understood by the Scottish Enlightenment thinkers. Gebhardt's thesis—that, as purification, awakening, and reformation, the American Revolution was genuinely revolutionary—is, of course, controversial and deserves to receive considerable attention and serious debate. Some may wonder at the extraordinary emphasis he places upon John Adams—to the comparative neglect of James Madison and Thomas Jefferson, for example—but whatever particular reservations one may have, there is much to learn from Gebhardt's fresh and arresting interpretation of Adams.

According to Gebhardt, the "crisis" of Americanism is the emergence of the atomized self and the gradual disintegration of the "public philosophy." The Lockean, individualistic, acquisitive mentality gains the upper hand over republicanism with its commitment to the common good and biblical religion with its commitment to "one nation, under God." The remarkable economic and material advance of our country has been bought at the price of almost destroying the very social fabric which made such an advance possible, he contends. Periodic attempts to "return to the origins" of the Revolution and inspire a new ethical-political awakening are discussed as well as the difficulties they encounter.

Although Gebhardt's study will be of particular interest to students of American history and politics, it also deserves to be read by political scientists concerned with the general problem of the role of a society's "civil theology" in undergirding its political system. Richly interdisciplinary in his approach, Jüergen Gebhardt has succeeded in writing a book about the United States which is both politically concrete and philosophically profound. There is here none of the philosopher's abstractness: Gebhardt is highly knowledgeable about American history and institutions. Let us hope it will be translated for a larger public. At that time the typographical errors in the English quotations can be corrected.

DANTE GERMINO

University of Virginia

Reason and Morality. By Alan Gewirth. (Chicago: University of Chicago Press, 1978. Pp. xii + 393. $20.00.)

Reason and Morality is an ambitious effort to reunify the "is" and the "ought," and to reunite analytical metaethics with traditional normative discourse. Alan Gewirth seeks to formulate a general theory of action, and to deduce therefrom both a rational justification for morality, and a universally applicable moral imperative. In attempting so huge a task, he has produced a system so abstract and obscure that its impact risks being limited to the stoutest adherents to analytical philosophy. Yet its content is inescapably political; Gewirth's system synthesizes many of the traditional issues of political philosophy, including rights and duties, obligation and dissent, the nature of the state and of voluntary associations, punishment, and welfare policy.

Gewirth asserts that nothing more than a definition of action and the rules of logic are needed to derive both a justification for morality and its specific normative requirements. To do so, he develops what he (rather ponderously) calls the "dialectically necessary method," which involves examining the logical implications of claims and judgments that are necessarily attributable to every agent. The key to this process is his definition of action. Action has two generic features, voluntariness or freedom, and purposiveness or intentionality. These features alone logically commit all actors to accept the moral principles derived from them. Behind this claim lies the conviction that the structure of action, because it involves the voluntary pursuit of purposes, is inherently normative. By this definition, and the dialectically necessary method, Gewirth claims to have bridged the gap between the "is" and the "ought," by demonstrating that the "ought" is implicit in the "is," the concept of action. He has not made an empirical bridge from the "is" to the "ought," but an analytic one.

Every agent, by virtue of engaging in action, is logically committed to accept the moral imperative on pain of self-contradiction. The reasoning is as follows: (1) Every agent judges his own purposes to be good, and hence judges as good the freedom and well-being that are the necessary conditions of his acting to achieve his purposes. (2) Every agent necessarily makes a deontic judgment claiming the right to freedom and well-being. (3) Since he makes this claim by virtue of nothing more than being an agent, he must accept the generalization that all prospective purposive agents have rights to freedom and well-being.

The controversial rights claims contained in the second and third steps merit further explanation. Since the agent considers his freedom and well-being good, he concludes that others ought not interfere with them. This "ought" is understood to mean "others have a duty not to interfere with them." This duty implies a correlative right or entitlement. This right does not rest on the existence of any community, government, or deity, nor on any particular attribute of the agent. Hence, any agent has the right to freedom and well-being by virtue of nothing more than being an agent. This last step allows Gewirth to universalize the rights claim. If one acknowledges that all agents have the right to freedom and well-being, one commits oneself to the Principle of Generic Consistency. This PGC requires that the agent refrain from harming or coercing any recipient of his action, and that he assist those who need assistance in achieving freedom and well-being if he can do so at no comparable cost to himself. This, then, is an egalitarian universalist moral principle, since it requires an equal distribution of the most generic rights. The PGC is both logically and normatively necessary, and is the supreme moral principle.

The final two chapters work out applications of the PGC for individual and group interactions. For political scientists, the most interesting aspects will be the indirect applications of the PGC developed in the final chapter, reflecting Gewirth's long-standing interest in political philosophy. Indirect applications are those in which the PGC is used to evaluate social rules and institutions governing multiperson activity. If the rules are thus morally justified, they are then binding on persons to whom they apply even when they burden freedom and well-being. Social rules may be justified either as extensions of freedom (procedural justifications) or as contributing to well-being (instrumental justifications). Procedural justifications apply to institutions based on consent, such as voluntary associations. The state, in contrast, is justified instrumentally, as an institution necessary for well-being; the existence of the state need not be based on consent. However, to be consistent with the PGC, a state must have a constitution which ensures consent in the selection of officials and of public policy. Gewirth argues that the body of criminal law ("the minimal state") merits different justifications from laws concerned with welfare functions ("the supportive state"). The former is logically mandated by the PGC and does not require consent; the latter are not strictly derivable from the PGC and thus must be based on consent. Readers are likely to find the political applications appealing, although some conclusions are counter-intuitive, such as Gewirth's insistence that to violate the criminal law is to contradict oneself. Overall, the insights of this chapter finally reward the persistent reader.

Persistence is indeed required to struggle through this dense book. Even the reader who enjoys metaethics will not find *Reason and Morality* pleasure reading. Gewirth's style is so turgid that it would bring tears of mirth to the eyes of Jeremy Bentham. He invents more principles than even John Rawls dared; in his hands, the Golden Rule becomes "the principle of appetitive-reciprocal consistency." Nothing is said clearly if it can be said awkwardly, ponderously, and with multiple noun clauses that assault the English language.

The content of Gewirth's argument will provoke considerable controversy, especially the logic by which he derives rights claims. Whether the logic ultimately stands or falls, it should promote serious thinking about the nature of rights and obligations. In summary, this forbidding book offers a significant contribution to political thinking. If its import is not recognized, the discipline will be the poorer, but at least part of the fault will lie with an author who intimidates rather than invites his reader to follow his difficult path.

BETTE NOVIT EVANS

Creighton University

Social Science and Utopia: Nineteenth-Century Models of Social Harmony. By Barbara Goodwin. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. ix + 220. $22.50.)

The subject of this book is four thinkers who believed they knew the social arrangements that would make people happy. To my mind, the idea that happiness is a condition, and furthermore, that it is a condition that can be established once and for all, approaches the ludicrous. One would have thought that happiness was simply not like other ends for which the institutional preconditions or embodiments could be known or plausibly conjectured, for example, freedom, national power, or the absence of poverty. Happiness comes as it can, and quickly goes. Surely there is something humorless—perhaps, even, sinister—in laying down detailed prescriptions for happiness, for happiness as a more or less steady state. Yet Barbara Goodwin sees Godwin, Saint-Simon, Fourier and Owen in this light, and tries hard to prevent us from judging their enterprise as intrinsically foolish.

Goodwin is not especially interested in showing that these thinkers' practice-minded utopianism sets them apart from most earlier writers commonly considered utopian. After all, it is one thing to imagine a happy world, but quite another to offer the unhappy world, in a completely literal-minded way, a blueprint for a happy world, with the hope that at least a few will start to live the happy life someone else has imagined for them. These literal utopians were not content merely to expand the feeling of possibility or to sting the world with their reproaches. They were out to change the world by force of argument. Goodwin only touches on this distinction between her four subjects and most of their predecessors. Failing to take fuller notice of this point, she therefore cannot account, in a thorough enough way, for her subjects' overall distinctiveness in relation to the body of utopian speculation they inherited. The four writers, children of the Enlightenment, as Goodwin rightly says, were also witnesses to the beginnings of industrial capitalism. That means they were possessed by the sense that unless something radical was done, the great benefits deriving from the growth in human capacities would be accompanied by great horrors. Perhaps the horrors would be greater than the benefits. (I do not mean to say, of course, that this sense was present with equal vividness in the four.) Desperation thus was mixed with hope; each intensified the other. The upshot was blueprints. For us, the upshot must in truth approach the ludicrous. A bit of historical empathy could help to stifle our derision; once we are past derision, however, is there a reason for spending some time with these writers?

Goodwin's announced strategy is to take these four seriously because their enterprise was intimately related to the growth of modern social science. It is an odd strategy, even though Saint-Simon had a large role in this ambiguous endeavor. Still, even if the endeavor were not ambiguous, and even if all four had played a large role, that would not guarantee that we now should take them seriously—that we should read and ponder them.

There must be a better reason to read them than their possible link to modern social science. What may we learn from them, even if we reject the aim of happiness? They may be full of thought about human nature and the relations between social institutions; they may contribute to our sensitivity to injustice; they may energize the wish to change and reform; they may even give hints about the direction which change and reform should take. Best of all they may give impressive examples of utopianism's most valuable effect: an enhanced understanding of the actual world which is brought about by the typical utopian perspective of estrangement from it. If we forgive them their passion to prescribe pedantically for happy lives, we may allow them to instruct us as non-literal and ironic utopians do—like, say, Plato or More or Swift or Samuel Butler.

It turns out that Goodwin, despite her title, is mostly interested in the utopian wisdom of the four writers. The link to modern social science is discussed, but not at much length. Her basic concern is with what Godwin, Saint-Simon, Fourier and Owen have to say about the human condition. She does not see their commitment to the project of human happiness as barring them from achieving some insight into human experience. She suspends her disbelief. She says, "These utopians were materialists, naturalists, perfectibilists and neo-Newtonians: they believed in material causes of social events, believed that society and men were natural objects to be studied by methods analogous to those of the natural sciences, believed in human perfectibility, and longed to unearth a principle of universal harmony operating in society with the efficiency and totality of Newtonian attraction" (p. 11). The usefulness of this book consists in showing how on such a morally unattractive and philosophically crude base four thinkers helped to form a new sensibility. They challenged individualist liberalism (itself still only partly formed) in the name of hope for a life more fraternal or collectivist or, at least, less competitively self-seeking. That sensibility establishes their claim to the attention of posterity. The project of human happiness inflamed by an awful dose of scientism should defeat such a claim, but does not quite.

GEORGE KATEB

Amherst College

Marx's Social Ontology: Individuality and Community in Marx's Theory of Social Reality. By Carol C. Gould. (Cambridge: MIT Press, 1978. Pp. xxvi + 208. $15.00.)

In her introduction Carol Gould claims to present Marx "for the first time as a great systematic philosopher in the tradition of Aristotle, Kant and Hegel" (p. xi). Though this claim is obviously exaggerated, *Marx's Social Ontology* is certainly the first work by an American to deal systematically with the *Grundrisse,* the voluminous notebooks translated into English in 1973. Gould's essay is coherent, well written, and should be accessible to the non-philosopher.

The first chapter, "The Ontology of Society," shows persuasively that for Marx the fundamental entities of society are individuals in social relations, that it is a mistake to treat society as a simple composite of individuals or as a complex network of relations. Gould shows the importance of the Hegelian dialectic in Marx's understanding of social development and does a masterful job of reducing great quantities of material to a few pages. Though with Hegel Marx speaks of various stages of history, his "projection does not have the force of a logical deduction or of a historical prediction" since the transition from one set of social relations to another depends on human choices and actions (p. 21). By developing these themes in terms of "internal relations," Gould successfully corrects earlier kindred accounts of Marx and points to the latter's appreciation of Aristotelian ontology.

In her second chapter on "The Ontology of Labor," Gould reconstructs Marx's view of labor as social self-creation and explains how in capitalism the objectification of the self common to all social systems becomes an alienation in which individuals lose control of their creative powers. In the *Grundrisse,* Marx makes clear that this change is to be explained in terms of political economy (the necessity of selling one's creative capacity, one's labor-power) and that a psychological or anthropological account of the phenomenon will mistake symptom for underlying disease. Gould shows how Marx's model of alienation and objectification, initially drawn from Hegel, is again modified by an Aristotelian emphasis on the reality of the individual and the independence of the object.

Chapter 3, "Toward a Labor Theory of Cause," is perhaps the most technical, but also the most important for social scientists. Gould shows Marx to have an original understanding of social causation which makes any mechanistic interpretation of his work totally inadequate. Marx locates causality not within the domain of natural objects or events, but in the realm of human activity, with labor again serving as the active mediator between form, purpose, agency and the objective conditions for the actualization of our intentions. Since the forms of human activity are historically variable for Marx, the ways in which causality appears and is understood vary from one historical stage to another, until finally under capitalism it appears as an external relation producing invariable patterns of behavior. Gould's analysis is especially important in that it could provide a link between continental thought and contemporary analytical work in the natural and social sciences (von Wright's analysis of human activity and the discovery of causal principles comes to mind).

In chapter 4, "The Ontology of Freedom," Gould distinguishes Marx's view from the traditional view of freedom as the absence of external restraints or as a property inhering in the subject's will. Freedom becomes both a presupposition and a product of our activity, the source of value and the highest value as an end in itself, "a creative, self-transcending, historical, self-realizing" social process, which accounts for newness in history (p. 127).

In the final and longest chapter, "The Ontology of Justice," Gould argues that Marx's theory of social reality presupposes a conception of justice understood in terms of concrete forms of social relations. Though Marx insists on the historical character of our norms, he is neither an historicist, nor a relativist. In analyzing relations of domination and the "mutuality" characteristic of a just society, Gould adeptly uses the category of reciprocity, so increasingly central to contemporary social theory. Capitalist relations of production presuppose free and equal agents, who exchange equivalent values in the market (wages for labor-power), but they disguise non-reciprocal relations of exploitation in the sphere of production, where workers surrender control of their creative capacity and ownership of the products generated when that capacity is put to use. The just society would allow for positive freedom, the fullest self-realization of social individuals who recognize each other as equal not only in a formal and instrumental way, but as ends in themselves.

Questions can be legitimately raised about any of several notions found in Gould's work: non-reciprocal, internal relations or ex post facto necessity, for example. But to ask them in a short review might leave the impression that we were speaking of analytical shortcomings. *Marx's Social Ontology* is a philosophical reconstruction of Marx's entire system. Questions will inevitably arise about its adequacy, but answers to these questions will provide insight into that system and gratitude to the person whose work has provoked them.

GEORGE ELLARD

Dickinson College

Leon Trotsky. By Irving Howe. (New York: Viking Press, 1978. Pp. viii + 214. $10.00; New York: Penguin Books, 1979. Pp. viii + 214. $2.95, paper.)

Leon Trotsky has fascinated generations of non-communist Western intellectuals for half a century partly because he seems to embody both the triumph and the tragedy of the Russian Revolution. The early opponent of Lenin helped plan the Bolshevik insurrection, create the Red Army and win the Civil War; the vacillating head of the "August bloc" became Foreign Minister, architect of "war communism" and Lenin's preferred successor; the cultured intellectual who stood for workers' power and inner-party democracy defended the accomplishments of the revolution against a usurping party-state bureaucracy; the outsider who had arisen to the pinnacle of power became the hunted exile; the "superindustrializer" relentlessly exposed Stalinist totalitarianism while urging his scattered and isolated followers to extend their "critical support" to the Soviet Union. All told, Trotsky is often presented as one of the towering heroes—and one of the most poignant victims—of history.

But "history is a bitch," remarks Irving Howe ironically at the end of his latest contribution to the voluminous literature on Trotsky—a sophisticated "political essay" whose occasional superficiality is more than offset by the kind of insights one has come to expect of him. The "Modern Masters" format does not cramp his style. Howe manages to say what he wants to say.

He presents Trotsky as a man of great intellectual abilities and considerable personal courage whose fundamental, if understandable, tragedy was rooted in his obstinate refusal to reexamine—and renounce—his fidelity to Bolshevism. Too doctrinaire for his, for Russia's and for the world's own good, Trotsky is sharply criticized by Howe on four grounds: his defense of the October insurrection itself (whose very success made inevitable the systematic application of terror by a minority party suddenly risen to state power and determined to transform the face of Russia and the world); his refusal to share power with other parties in an all-socialist government on the questionable presumption that only the vanguard Bolsheviks represented the "long-run" interests of the workers; his strict adherence to democratic centralism even while he was advocating freedom to form factions and defending what remained of inner-party democracy; and his stubborn defense of the Soviet Union whose socialist foundations remained intact despite their bureaucratic "deformations." Howe argues that it was this tragically inappropriate fidelity to Leninism which paralyzed whatever Trotsky might have been able to do in defense of the Revolution and democracy.

But this, the core of Howe's analysis, is precisely his most vulnerable claim—and the book's value resides chiefly in its presentation of a popularly held, if highly questionable, position. To take one example, Howe says that the theory of "permanent revolution," the "going-over" of the bourgeois to the proletarian revolution, was a decisive contribution to Lenin's own thinking. But there is abundant evidence that Lenin was not indebted to Trotsky on this score and that the related projections concerning the relationship between the peasantry and the proletariat and the possibility of building socialism in Russia in the absence of successful European revolutions had little, if anything, to do with Lenin's thinking. The same might be said for such issues as the Brest-Litovsk treaty, the role of the trade unions, the New Economic Policy, the nature of the Communist party, the meaning of the phrase "hegemony of the proletariat," the "national question," and others.

All the leaders of the modern communist movement have tried to answer the challenge of constructing a Marxism which remains international in content even as it takes national forms. That they have not all done so in the same fashion or with the same success is clear. Trotsky proved no more equal to the task than most others, and Howe's portrayal of him as an overly rigid Bolshevik engaged in heroic combat with Stalinism—the very incarnation of evil—does not help the reader approach the deeper issues of Trotsky's work. It is an interesting work for all that; and, more than an examination of Trotsky, it also illustrates the dilemmas which have confronted anti-communist socialists since 1917. In this sense, *Leon Trotsky* is about the theoretical problems of its author at least as much as those of its subject.

JOHN EHRENBERG

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Pride and Solace: The Functions and Limits of Political Theory. By Norman Jacobson. (Berkeley: University of California Press, 1978. Pp. xv + 166. $10.00.)

How does one review for an academic journal a poetic, frequently metaphorical essay, a work filled with startling images and sometimes outlandish linguistic constructions? One cannot look exclusively for logical consistency, because, as Norman Jacobson claims, poetry is not logical. Nor can one deal with the accuracy of the sometimes novel and imaginative interpretations of Machiavelli, Hobbes, and Rousseau, for, again, the author states that he is looking for meaning rather than truth, presumably as much in his readings of the works of others as in his own life as a political man. Political theory for the author is persuasion mixed with intimations, intimations "of what persists within the political consciousness of Western humanity" (p. xiii). And it is this which the author tries to offer us in his rapid and sharply focused survey of Western political theory from Machiavelli's state to Camus' rebel. It is a persuasive and powerful plea to reject the solace of uncertain truths, while accepting the uncomforting solace found in the writings of Orwell, Arendt and Camus. Theirs is not the solace of truth and order, but the solace of those who have experienced and put into words the tensions and the nightmares of being human, but who nevertheless refuse indifference and who retain compassion.

Machiavelli, Hobbes, and Rousseau do not satisfy this craving for solace which Jacobson feels. The solace they give is tainted and one-sided. They give people the solace of order, but take away their humanity. Machiavelli's order is based on the conquest of inclinations to be good, Hobbes' on the amputation of private consciences, and Rousseau's on the deception of the legislator. In Jacobson's analysis of what he describes as three founders of the modern state, his interest focuses frequently and most importantly on the relationship between subjects and rulers, between the Italians and their prince, between the fearful members of Hobbes' civil society and their all-powerful sovereign, between the natural man made citizen and the legislator. It is the legislator, the Moses of political thought, who provides solace for his subjects, solace in the form of order and security, and, in Rousseau, having the additional support of a sense of justice and equity. Jacobson's thesis, though, is that the nineteenth century destroyed this relationship and left political beings in a pre-Machiavellian condition, "deprived of the solace of mystery, authority, or pre-ordained destiny, alone, groping [their] way through a labyrinth of uncertainty" (p. 129).

The argument is broad and consequently ignores or does not deal with contradictions or questionable details. In order to emphasize points, to paint with striking chiaroscuro, Jacobson oversimplifies and obscures tensions which would undercut the power of the picture. Can we really compare post-Rousseauian man with pre-Machiavellian man and simply ignore the impact of both Christianity and the Reformation? With regard to individual authors, broad generalizations again dominate. *The Prince* is a grand plea to Lorenzo, or really the seduction of the politically ambitious man; the seriousness or the scope of Machiavelli's plea remains unquestioned. The *Leviathan* presents Hobbes as the Creator, creator of a new man bound to the sovereign through language and a fear of himself, but oblivious of the concepts of obligation and representation. Rousseau's general will is the union between the state of nature and civil society, but is there not a far more radical and complex transformation entailed in the move to civil society than simply an uncertain and insecure unity between the two? But then again, one is called back; what else is the role of art but to focus on one element of human nature, to the exclusion of the rest, in order to sharpen one's understanding and awareness of what we ordinarily do not see in the mix of complexity? On this level the book is a jewel.

On a lesser scale there are minor inaccuracies which offend but do not necessarily detract from the overall effect. For instance, Hobbes did not write a work entitled *Body, Man and Citizen* to which Jacobson refers on p. 67. The major quotation which he uses comes from part I, chapter 9 of *The Elements of Law*. Furthermore, Hobbes' first literary project was not a translation of Homer (p. 91); it was a translation of Thucydides. This does not destroy Jacobson's claim that Hobbes was a lover of heroic poetry and as aware as Plato of the power of poetry, but it does create some concern in readers who, while being pulled along by the poetry, do not want totally to relinquish their hold on history.

This is not a scholarly book in the limited academic sense of having accurate footnotes (which are largely absent until the last chapter and the discussion of the twentieth-century authors); nor is it a scholarly work in the Greek sense of the term—something which emerges from one's leisure time. It is a passionate book written by one who feels the need to confront, deal with and then put into words the dilemmas, as he and Camus see them, of modern political beings who must set limits and retain their humanity in a chaotic world not in the least conducive to these goals. The quibbles which one may have about particulars in the book often pale before its rhetorical and poetic power.

ARLENE W. SAXONHOUSE

The University of Michigan

The Study of Politics: A Collection of Inaugural Lectures. Edited by Preston King. (London: Frank Cass, 1977. Pp. xiv + 322. $27.50.)

Inaugural professorial lectures are British occasions without an exact American analogue. Nevertheless, we have ceremonial events for which statements of intellectual faith are expected. Participants in such events will be sympathetically interested in the 16 inaugural lectures delivered by British professors of politics and collected in this volume. Others may find the book revealing as academic history. Newly appointed British professors ordinarily present their plans for future study and development. Such plans are collegial as well as personal when professors become departmental heads.

Preston King, the teacher of politics who edits the volume, has included certain frequently cited and controversial essays, notably Michael Oakeshott's "Political Education" and Isaiah Berlin's "Two Concepts of Liberty." Harold Laski, Ernest Barker, Denis Brogan, G. D. H. Cole, K. C. Wheare, and Max Beloff, established British scholars who became well known in the United States during the second quarter of this century, are also represented, as are the slightly later figures of J. C. Rees, Howard Warrender, J. H. Burns, W. H. Greenleaf, M. M. Goldsmith, A. H. Birch, Maurice Cranston, and Bernard Crick. Of the 16 lectures, 1926–1971, four come from Oxford, three from the London School of Economics, and two from Cambridge. Thus the three places with earliest politics chairs are well represented in the collection. There is no suggestion that the other seven lectures represent British political studies, or even British professors, at all of the remaining three dozen or more universities where politics is taught. Conspicuously absent, for example, are lectures from Strathclyde and Essex, where (along with Nuffield College, Oxford) something like American behavioral political science has developed. Perhaps professors at these universities have not given inaugural lectures.

Whatever the reason for the omission of the now-familiar American style, much can be said for a volume that emphasizes an approach to the study of politics that is primarily philosophical and historical. Neither the exponents nor the restive critics of that approach are likely to deny that it has been dominant in Britain during almost all of the first half-century of politics as an academic subject. Its ascendancy may slowly be drawing to a close. Certainly many British scholars have now forsaken the old essay-writing tradition for rigorous quantitative analysis as well as for other empirical research methods. But good reasons remain for reading work in the traditional mode. Several of the philosophical lectures are important in their own right; Oakeshott's and Berlin's, and also Cranston's "Politics and Ethics" lecture of 1971, are full statements of important ideas. Crick's lecture in 1966 is a characteristically lively response to Berlin's. Moreover, the collection is itself a useful contribution to intellectual history. It tells us something about the British reluctance to treat politics outside of a humanistic tradition. The very name "political science" is usually avoided, or, now and then, treated as an inappropriate American usage. Max Beloff reacts explicitly against behavioral methods; otherwise such methods tend to be ignored.

The editor's introduction directs attention to a change over time in the nature of the inaugural lectures. The early ones, before 1951, try to mark the boundaries of what is still a new academic discipline, and the later lectures, taking the boundaries more or less for granted, are devoted to the professors' own particular subjects. Accordingly, even within a strongly humanistic tradition, there are signs of the growing specialization that has become so familiar within American political science and indeed within all of our established academic disciplines.

It would be wrong to conclude this review without noting exceptions to the generalization about the volume's concentration on the philosophical and historical tradition in the study of British politics. K. C. Wheare's lecture is one of a few contributions reflecting a concern with the study of governmental institutions. More striking, however, is the scope of intellectual interests displayed in Harold Laski's inaugural lecture at the London School of Economics in 1926. Although he declared his own main interest to be the history of political ideas, Laski also asserted, in discussing what was known about the House of Commons, "that in the place of large-sounding generalizations that have never been tested we badly require quantitative answers. . ." (pp. 11–12). Premature British behavioralism?

LEON D. EPSTEIN

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The Essential Tension: Selected Studies in Scientific Tradition and Change. By Thomas S. Kuhn. (Chicago: University of Chicago Press, 1978. Pp. xxiii + 366. $18.50.)

It is now over 16 years since Thomas Kuhn's *The Structure of Scientific Revolutions* was first published. During this period Kuhn's views have been the subject of a continuing, often virulent debate, but in spite of the hostility it has aroused in some quarters, the book has had a deep impact on our understanding of "science" and what is involved in the scientific study of politics. His latest book, *The Essential Tension,* is a collection of essays, 12 of which were published between 1959 and 1976, and 2 unpublished essays written in the same period. Together these essays provide an overview of the development of Kuhn's ideas, several studies in the history of science, and an amplification and clarification of some of the ideas developed in *The Structure of Scientific Revolutions.*

Judging from the reception of Kuhn's earlier work, the essays that will be of greatest interest to political scientists may well be his restatement of the concept of a paradigm in "Second Thoughts on Paradigms," his clarification of the role of values and "subjectivity" in theory choice in "Objectivity, Value Judgment, and Theory Choice," and his critique of Popper in "Logic of Discovery or Psychology of Research." These essays, together with his "Postscript" to the second edition of his *Structure of Scientific Revolutions,* show that Kuhn's account of science and scientific revolutions does not have the irrationalist implications often attributed to it. In other essays Kuhn continues to argue that significant changes in a scientific theory, including the rejection of one theory and its replacement by an entirely new one, cannot at the time be fully controlled by the evidence, or completely determined by any rules of rational scientific procedure. But this does not mean that scientific change is a matter of "mob psychology" or anything of the sort. On the contrary, such decisions must be justified: "Scientists may always be asked to explain their choices, to exhibit the bases for their judgments. Such judgments are eminently discussable, and the man who refuses to discuss his own cannot expect to be taken seriously" (p. 337). The bases for these decisions are scientific values such as accuracy, consistency, scope, simplicity, and fruitfulness. These values are not so well-defined that they can dictate the choice between two theories, but this is a virtue rather than a defect, because the practice of science requires the possibility that rational people may disagree about what theories ought to be accepted, permitting work to proceed on competing theories. By this means, premature closure is avoided, and the consensus that results as one theory increasingly comes to prove its worth vis-à-vis its rival can be seen to be a rational consensus.

In "Second Thought on Paradigms" Kuhn provides much-needed clarification of what has come to be one of the most overworked terms in our metascientific vocabulary. Here Kuhn argues that "paradigm" should not be used to refer to the whole set of elements which a discipline (or school) shares, and which accounts for the relatively unproblematic nature of communication within it. This set should be thought of as a "disciplinary matrix," and Kuhn offers a valuable discussion of three crucial elements of any disciplinary matrix, including symbolic generalizations, models, and exemplars. "Exemplars . . . are concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic" (p. 298). Thus, Kuhn proposes to return to his original, and basic sense of "paradigm," and offers a stimulating and important discussion of how such exemplars function in scientific practice.

Although Kuhn's more philosophical essays are apt to attract the most attention from political scientists, his reflections on and his studies of the history of science should also be of great interest. In his preface and in the historical and historiographic studies, Kuhn stresses the hermeneutic aspects of his field, the need for historians to understand the theories they are studying in terms of the concepts and principles possessed by the scientists who originally formulated them. In these essays we see Kuhn as a fellow social scientist seeking to understand a form of human behavior—the practice of science. As political scientists attempting to understand the political actions of men and women, our position is not unlike that of the historian of science, for our first task must also be an interpretative one, to grasp the sense these actions make in terms of the actors' own self-understandings. Unfortunately, although suggestive, Kuhn's discussion of what such interpretation involves is very brief and sketchy. In particular, he does not relate the problem of interpretation faced by the social scientist, or historian of science, to the problem of interpretation faced by the natural scientist. Where social scientists must make sense out of the behavior of their subjects, natural scientists must understand theories that have been formulated in terms of an alternative "paradigm" (in the broader sense of the term). On the face of it, both of these situations appear to raise similar issues of mutual understanding and intelligibility—problems of meaning, translation, incommensurability, and so forth. These issues have long been of concern to social scientists, historians, and philosophers who have studied the problems of the social sciences and humanities, and so it can be hoped that there is room here for valuable cross-disciplinary work. Indeed, after a long period in which social scientists have turned to the philosophy of natural science for guidance, it may well be time for them to contribute to the solution of problems within that field. The essays collected in this volume are a contribution to that enterprise.

J. DONALD MOON

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The Spirit of Liberalism. By Harvey C. Mansfield, Jr. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xii + 130. $13.50.)

This book contains six essays. Three are primarily book reviews: one of Lowi's *The End of Liberalism,* one of Reich's *The Greening of America* and Dahl's *After the Revolution?* and one of Rawls and Nozick. One was a bicentennial *pièce d'occasion* on "The Right of Revolution," while the remaining two lament the decline of contemporary liberalism.

Mansfield speaks of the reader "catching his tone" (p. ix) and this is indeed distinctive: "feline" is I think the word. ("Waspish" would be too gross a pun.) Although a constant diet of irony and innuendo becomes cloying, some of Mansfield's effects are worthy of A. E. Housman himself, the master of us all in the gentle art of academic bitchery. For example, in Mansfield's review of *After the Revolution?* he sets things up by observing that the book is a "Yale fastback" and quoting the publisher's flackery for this concept. He then notes that Dahl crosses "the boundary between empirical political science and political philosophy . . . thus finding himself, so to speak, in the ladies' room" and continues: "It may merely be that he who writes a Fastback has no time for the fact-value distinction. . ." (p. 62).

Occasionally, the mask slips, and Mansfield starts to sound like a character invented by Kingsley Amis, as in his nice thought that busing will merely speed up the process by which the Negroes (as he calls them) acquire the middle-class virtues while "the white middle-class suburbanites, especially the young ones, . . . adopt some of those ways of the Negro which derive from his former status as slave—a slouchy posture, a shuffling gait, shiftless habits, gaudy attire, throbbing music, and the various addictions" (p. 36). Or is this another mask? Having sent up everybody else, is Mansfield sending himself up here? Who knows? Does even he?

Reviewing the book is a hazardous business because of the element of what Nixon and his friends called "deniability": any rendering of its content must take account of what is insinuated as well as what is stated, but how does one then reply to the charge of "I didn't say that?" Having admitted that risk, I shall try to summarize the main contentions by saying that Mansfield combines the Agnew diagnosis of the media with the Kristol diagnosis of the academics: for Mansfield both are guilty of biting the capitalist hand that feeds them, and they are joined in Public Television, which excites his special contempt. The enemy is "West Coast university and New York literary activism" (p. 50), or more concretely, the New Left and the radical feminist movement (pp. 22–27). Members may be readily identified as men trying not to be gentlemen (p. 14) and women pretending not to be ladies (p. 26).

But what is Mansfield actually in favor of? He is, he says, "a friend of liberalism" (p. vi). Failing a revival of the thought of "the ancient writers who [by denying that all authority is artificial] constitute the true alternative to liberalism" (p. 50), one can still choose between its "moralistic" and its "mundane" tendencies. Mansfield prefers the latter and lauds the tough-minded liberalism of Locke and Madison for its frank recognition of the force of self-interest in human affairs, and for its analysis of large-scale societies as necessarily divided into an active elite and a passive mass. Thus, Lowi "does not face the necessity of social aristocracy in our liberal society and hence does not appreciate the necessity of disguising it in our liberal democracy" (p. 38). Tocqueville rides again: the egalitarian "spirit of democracy" must ever be opposed to the elitist "spirit of liberalism."

How did contemporary intellectuals go so astray? Mansfield, missing the point of Rawls' theory, which uses the notion of constrained maximization to make a moral argument, accuses them of being concerned with the lot of the disadvantaged out of a self-deluding fear of being among the worst-off (pp. 100–01). The "unassertive 'apathetic' many" (p. 9) have rather simple desires for material goods and "living as they please" (Earl Butz put it more vividly) and demands for equality or participation made on their behalf are merely a bid for power by radical elitists (pp. 14–15). Intellectuals "have been more democratic in their demands than the democrats" (i.e., the untalented mass) and have "lost their sense of community with businessmen" (p. 14). But are freedom to accumulate and freedom to publish indissolubly linked? Mansfield offers no serious argument for this. Copyright is, indeed, one form of property (p. 13), but any reasonably spry intellectual of leftist inclinations should be able to dispose of that little debating ploy.

BRIAN BARRY

University of Chicago

On the Social Contract, with Geneva Manuscript and Political Economy. By Jean-Jacques Rousseau. Edited by Roger D. Masters and translated by Judith R. Masters. (New York: St. Martin's Press, 1978. $12.95, cloth; $4.95, paper.)

Roger and Judith Masters, who have already placed us in their debt with a fine edition of Rousseau's *First and Second Discourses,* now increase that obligation with a new version of *The Social Contract,* to which they have appended the *Discourse on Political Economy* and (more importantly) the first complete English translation of the "First Version" or "Geneva Manuscript" of *Du Contrat Social.* The translations are extremely faithful, though they perhaps abandon a little of the grace and elegance that Frederick Watkins managed to preserve in his equally reliable 1953 rendering of *The Social Contract;* and Roger Masters' introduction is admirably wide-ranging and comprehensive for so brief an essay, though it is perhaps, on occasion, a little elementary for those already acquainted with Rousseau ("Rousseau . . . represents the generation which rejected the principles of the old order without being able to overthrow it . . . this general movement is often called the 'Enlightenment' "). This is offset, however, by a most judicious treatment of Rousseau as popular-sovereignty theorist, and still more by a skillful elucidation of that central but elusive Rousseauean notion, the general will—a notion that Roger Masters rightly calls the "most notable" of Rousseau's enlargements of our political vocabulary.

It is because the "notability" of Rousseau's *volonté générale* is so clear to Masters that he has seen fit to include the *Première Version* or "Geneva Manuscript" of *Du Contrat Social,* the significance of whose second chapter he properly insists on. In that second chapter, which Rousseau suppressed, together with the rest of the *Première Version,* Rousseau takes up Diderot's argument (from the *Encyclopédie* article, "Droit Naturel") that there is a "universal" general will of and for the entire *genre humain*—a universal *volonté générale* which "never errs," and which is based on "reason" in "the silence of the passions" (a phrase that Diderot borrows from Malebranche). In the "Geneva Manuscript" Rousseau flatly rejects this universality: "There is no natural and general society whatever between men; . . . it is certain that the word *human race* presents only a purely collective idea to the mind, which presupposes no real union between the individuals who comprise it." And Rousseau, having insisted that what is *général* for Sparta or Geneva is *particulier vis-à-vis* the rest of the world, finishes by complaining of "pretended cosmopolites" (e.g., Diderot) who, "justifying their love of country through their love of the human race, boast of loving everyone, to have the right of loving no one." Roger Masters gives full weight to this important manuscript in his introduction, and fleshes it out with valuable "editor's notes."

It is in one of these notes that Masters makes his only seriously disputable claim—namely that the notion of "general will" was used before Rousseau, not just by Diderot, Malebranche and Fontenelle (as is true), but also by Pufendorf. In his tenth "editor's note" to the "Geneva Manuscript," Masters, apparently following de Jouvenel and Hendel, suggests Pufendorf as an early shaper of *volonté générale;* but here Masters, like Hendel, appears to have been misled by Barbeyrac's French translation of Pufendorf into thinking that the notions of *volonté générale* and *particulière* are really to be found in *De Officio Hominis et Civis* (1682). What is unmistakably plain is that Masters, like Hendel, cites Pufendorf neither in the original Latin nor in an accurate English translation, but only in Barbeyrac's French version of *De Officio Hominis,* which was published in 1718 as *Les Devoirs de l'Homme et du Citoyen.* Now Barbeyrac used the notions of *volonté générale* and *particulière*—developed initially in theological writings of Pascal (*Écrits sur la Grace*), Arnauld and Malebranche—in "rendering" *De Officio Hominis* into French; his version is a transmogrification of Pufendorf, not a mere translation. Thus Pufendorf's plain phrase, "if each subjects his will," becomes in Barbeyrac "if each subjects his *volonté particulière*"; and Pufendorf's straightforward "the will of all, collectively and singly," is turned by Barbeyrac into "the positive *volonté* of all *en général* and of each *en particulier.*" So Barbeyrac's filtering of Pufendorf's quasi-Hobbesian thought through the prism of French theology makes him *look* like a precursor of Diderot and Rousseau—but only in French.

This, however, is comparatively unimportant; what matters is that Roger and Judith Masters have appreciated, and given to us in a fine English version, one of Rousseau's most important and neglected political writings. For this one can only be grateful.

Patrick Riley

University of Wisconsin

The Critical Theory of Jürgen Habermas. By Thomas McCarthy. (Cambridge, Mass.: MIT Press, 1978. Pp. xiii + 466. $19.95.)

Jürgen Habermas must be accounted one of the most suggestive and interesting thinkers currently at work in the social sciences. Yet understanding him is no simple matter. His thought grows out of an exacting tradition. A systematic and speculative thinker in the style of German idealism, he assumes his program and methodological convictions from neo-Marxism, particularly the Frankfurt School critical theory of Max Horkheimer and Theodor Adorno. His prose is often opaque, joining the inscrutable allusiveness of Adorno with a jargon-riddled verbal eclecticism that is uniquely his own. His system is imperial in ambition and encyclopedic in scope, recasting other theories in its own mold, and requiring of the reader some acquaintance with not merely Kant, Fichte, Hegel, Marx, Dilthey, Weber and Freud, but also C. S. Pierce, H. G. Gadamer, Talcott Parsons, Wittgenstein, J. L. Austin, John Searle, Noam Chomsky, Jean Piaget and Lawrence Kohlberg. Constantly assimilating new theories and ideas, largely through a steady stream of critical essays, Habermas is always expanding, refining and reformulating his system, which he has come to call, with misleading modesty, a "research program." This vast—and growing—program has already generated a vertiginous superstructure of propositions that are prima facie implausible: for example, the claim that the relations within groups pursuing political enlightenment ought to be analogous to the therapist-patient relationship in psychoanalysis, or the hypothesis that the history of the species passes through universal stages of cognitive and moral development analogous to the stages of individual development observed in advanced industrial societies. Still worse, the avowedly programmatic status of the system makes evaluation treacherous. Faced with an unfinished "research program," criticism runs the risk of seeming premature, interpretation the risk of seeming one-sided, if not obsolete.

Despite these formidable obstacles, Thomas McCarthy has managed to write a relatively lucid book that is unquestionably the most cogent account in any language of Habermas' work to date. His professed aim is "to provide a reliable framework," from a "standpoint internal to Habermas's thought," for "the critical reception of [his] work in the English-speaking world" (pp. x, xi). But what we get is something more than a loyal and sophisticated apologetic. We also get a tacitly authorized "rational reconstruction" of Habermas' system—a restatement that, in virtually every respect, stakes its claims far more clearly and plausibly than anything Habermas himself has ever written.

This painstaking reconstruction is not light reading. For those who have never struggled through the original, it will not be of much help (they might start instead with Richard Bernstein's account in *The Restructuring of Social and Political Thought*). For those already acquainted with Habermas' work, however, McCarthy's book is indispensable.

What began as an attempt to justify and extend the critical theory Habermas inherited from Adorno has become an ambitious effort to show the existence of universal standards of truth and right. The foundation of this effort is the theory of communicative competence. It claims that "the human interest in autonomy and responsibility is not mere fancy, for it can be apprehended a priori," by elucidating the necessary structure of language (p. 287). To this "quasi-transcendental" theory, he conjoins (among other things) an account of ego development, a "rational reconstruction" of cognitive development in the species, and an analysis of capitalist "legitimation crises" designed to disclose latent social tendencies to redeem the claims of rational autonomy and discursive validity implicit in every speech act.

If one swallows all this whole, it would appear that both moral and epistemological relativism have been refuted, and that something like a neo-Marxist critical theory has finally been justified, without recourse to that discredited deus ex machina, the proletariat. Certain formal yet substantive concepts of truth and right are embedded in the very structure of competent speech, and this structure is unambiguous enough that "there is only one correct theory" of its phylogeny and ontogeny (p. 265). Accepting this account allows the comforting thought that history is a progressive "learning process" writ large, with a rational moral *telos* immanent in the language through which learning occurs. At the end of history (at least as a counterfactual "narrative foil") stands "the ideal speech situation," where uncoerced consensus emerges through unlimited dialogue about fundamental norms.

Clearly, these are bold claims that merit serious consideration. This McCarthy has given them. He convincingly dissects the early theory of "deep-seated anthropological interests," deftly untangles the subsequent theory of communicative competence. If these theories still do not carry conviction, it is not McCarthy's fault. One could not ask for an abler or more informed philosophical defense.

My main reservation, indeed, concerns McCarthy's tendency to mute his own misgivings. Although he is clearly uncomfortable with Habermas' surprisingly uncritical treatment of systems theory and Kohlberg's scheme of moral development, he confines himself to several sketchy and scattered paragraphs of comment. A little more historical context would also have helped: since the work of Horkheimer and Adorno is only discussed in passing, the false impression is left that Habermas virtually invented the idea of a "critical theory."

Minor criticisms aside, McCarthy has handsomely accomplished his principal aim. Moreover, the glosses on Habermas' critiques of behaviorism, functionalism, role theory, phenomenology, ordinary language philosophy and hermeneutics should be of value to anyone concerned with the philosophy of the social sciences. At the very least no one interested in critical theory can afford to ignore McCarthy's book. It is an impressive achievement.

JAMES MILLER

University of Texas, Austin

Game Theory and Political Science. Edited by Peter C. Ordeshook. (New York: New York University Press, 1978. Pp. xii + 627. $28.50.)

An abstract gold medallion, designed by the French artist Georges Mathieu in 1944 and entitled "Game Theory," is imprinted against the blue background on the cover of this volume. What would a medallion for political science look like? We are not given much of a clue. Eleven of the 19 essays in this volume have the word "game" in the title; none has any direct form of the word "political." My overall assessment of this volume is that the appropriate logical connective for the title is "or."

So much for my particular concern for the lack of any systematic focus on political (or even social) problems throughout the volume. Since we do not have a book which attempts to make game theory more relevant to politics through a reorientation of the former (Schelling is virtually alone in having broken this barrier) or to develop a unified approach to model building, we must evaluate this collection of essays through piecemeal evaluations of each article. Most of the "big names" in both formal political science and applied mathematics are here. Their primary contributions are generally reserved for journals, including this one. Thus what we have, for the most part, is a set of "remaindered" essays.

The articles in the first section, "Descriptive Theory," are either summaries or elaborations of the authors' other work in this area. Ordeshook and McKelvey and Harsanyi offer new solution concepts for cooperative and noncooperative games, respectively. The former article presents little that is new and the utility of the competitive solution still has to be demonstrated; Harsanyi's formal development is interesting (an extension of Bayesian decision theory to n-person games), but is so heavily dependent upon the notion that one or more players will make a strategic *mistake* that its applicability is unresolved. The essays of Lucas and Maceli and Schofield are also more relevant to game theory than to applied social science, although the former's discussion of the different types of domination may prove useful in the analysis of coalitional behavior. Robert Rosenthal offers a "model of cooperative games with binding commitments," a fascinating applied piece which unfortunately does not consider the prospect that a group may fail to provide a collective good. The Ferejohn et al. article is a direct application of game theory to legislative behavior, with supporting experiments. This is the most impressive piece in the first section, but it is marred by the authors' unnecessary assumption that a majority rule equilibrium within a legislature must be equal to an electoral equilibrium for each of the candidates; have they forgotten about patterns of districting?

Experimental studies (second section) often are so preoccupied with the details and procedures of the experiments that the underlying problem is almost lost. This problem is particularly acute in Vernon Smith's article, which finds some support for the optimal provision of public goods regardless of group size. Laing and Olmstead report a complex series of experiments which finds reasonable support for the McKelvey-Ordeshook competitive solution; their own ad hoc model does remarkably well, however, and it makes fewer predictions on outcomes. Isaac and Plott also employ a spatial approach to committee decision making under a "closed rule" (no amendments allowed); they offer a useful critique of bargaining set predictions and find that their subjects tended to select the outcome which was in the core of the voting game. Cores generally do not exist, but the Ferejohn et al. article in the first section suggests that this may be more of a problem for game theory than for committee decision makers.

The most interesting contributions are in the final two sections of the book, "Applications" and "Value Theory and Applications." The most ambitious and controversial article is an extended essay by Brock which employs supergames to "derive" a "new theory of social justice." This is not completely novel, as Taylor has tread this ground in *Anarchy and Cooperation;* however, Brock maintains that game theory can prescribe a set of ethics—and a rather conservative one at that. The other contributions on value theory are concerned with power indices. Owen (in apparent ignorance of the other contributions) and Roth develop rather abstract axiomatizations of the Banzhaf and Shapley power indices. Straffin, like Roth, also follows the lead of Dubey in axiomatizing these power indices, but provides much more straightforward interpretations in terms of key issues in democratic theory and gives most useful examples. This essay is a compendium of his results published in many places and should have served as a model for many other contributions. Brams and Lake present a slightly different axiomatization, also with political illustrations, but their model assumes that each outcome in a voting situation is equally likely and its utility is thereby limited. The other contribution to this section, by Shubik and Young, develops the nucleolus as a noncooperative game solution. Its primary interest in this volume is to provide a basis for Young's model of lobbying presented five chapters earlier. The latter contribution opens new ground in a field not sufficiently developed (although entrepreneurs have been studies). The present model needs to be extended to more than two lobbyists to be of interest to most students of politics. Young should concentrate more on developing the model of lobbying than in comparing the power of legislators who have been lobbied with those who have not (the Shapley and Banzhaf indices reappear).

The "Applications" section, in addition to the Young article, contains three important articles. Hinich demonstrates that the assumptions we make about voters' preferences (whether loss functions are quadratic or computed by absolute values) and their probabilities of voting at all determine whether the mean or the median will be the equilibrium point for a slightly modified spatial model of voting. Kramer presents a more difficult argument in establishing a theorem that maintains that under quite general conditions both candidates in a two-candidate election will have optimal *mixed* strategies and that when candidates are advantaged, optimal pure strategy (for the candidate or for the opponent) may exist. Kramer does not discuss how the optimal mixed strategy is to be operationalized. McKelvey and Howard Rosenthal develop a rather appealing model of electoral coalitions in French *apparentements,* one of the most politically relevant models to employ game theory, but then test the model (albeit too briefly) only with predictions from classical game theory. Not surprisingly, the results are mixed. The work of DeSwaan should motivate future testing of the coalitional model.

The text of the volume is set by photo-offset on inexpensive paper. There are typographical errors throughout the book and Figures 1 and 2 in the Ferejohn et al. article are nowhere to be found. Only in the printed preface is there any concern for margins and, of course, there is no index. These cost-saving devices reduce the price to $28.50, which is still considerably less expensive than other books which use far fewer mathematical symbols. Will esthetic considerations turn away potential purchasers? The press seems to have gambled that they will not. Yet I doubt whether I would purchase the book myself. In contrast to some of the more expensive volumes, it is not only in the margins that this book is uneven.

ERIC M. USLANER

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The Golden Lands of Thomas Hobbes. By Miriam M. Reik. (Detroit: Wayne State University Press, 1977. Pp. 239. $15.95.)

The Golden Lands of Thomas Hobbes is a biography, or rather, an intellectual biography; for, as its author points out, Thomas Hobbes did scarcely a thing throughout his 91 years besides think, write, and converse. With so little of an active life to record, Miriam Reik devotes the weight of her study to the impact of Hobbes' mature work on such contemporary types as jurists, political figures, moral philosophers, scientists, churchmen, and the English public at large. What that mature work consisted of she explains in three chapters on Hobbes' contributions to science, politics, and literary criticism. Finally—and for Hobbes scholarship most importantly—in a chapter analyzing Hobbes' introduction to his Thucydides translation, Reik makes the argument that no unbridgeable distance separates Hobbes the humanist from Hobbes the scientist.

It is somewhat customary among Hobbes scholars to take the publication of Hobbes' Thucydides as closing one stage in the philosopher's life, and his subsequent discovery of Euclid as opening another. Reik contends, to the contrary, that even the admittedly humanistic translation and its introduction reveal a concern for those very problems that are to characterize Hobbes' more scientific researches. The central issue raised by Hobbes in his introduction is the role of history in the conflict between reason and passion, or, pedagogically speaking, in the conflict between dialectics and rhetoric. Thinking that the proper use of language is essential for the apprehension of truth and for the undertaking of deliberate action, Hobbes commends Thucydides for his recognition of the linkage between language and political well-being. Hobbes also expresses approval of Thucydides' rigorous attention to the laws of causality and of his device of fictive orations. Here Reik astutely observes, making her point of continuity, that the historical method of hypothetical constructions corresponds exactly to Hobbes' later method in the physical sciences (p. 49).

It is to this scientific method that Reik next turns and to the consequences of Hobbes' encounter with Euclid. Hobbes endeavored to place politics on as sure a foundation as science, and he learned from Euclidean geometry the methodology of deriving necessary propositions from a few terms correctly defined. Having explained Hobbes' indebtedness to Euclid, Reik proceeds to analyze: his implacable resistance to innate ideas, or separate essences; his epistemological contention that human knowledge is limited to what human beings construct (with the attendant notion that politics can be scientific because the state is artificial); his doctrine of motion and its implications for sense perception and for mechanistic materialism more generally; his theory of the subjectivity of secondary qualities; and finally his running battle with Descartes over the question of dualism.

On the subject of Hobbes' new science of politics, Reik mainly confines herself to a discussion of the state of nature and the origins of civil society, the theory of sovereignty, and the friction between that theory and the legal traditions current at the time. She explains how for Hobbes natural right (self-preservation) supersedes any duties that can be ascribed to the laws of nature (seek peace) and how justice, obligation, and the concepts of good and evil are wholly conventional, owing their vitality to an independent power responsible for their enforcement. She points out further that because of the close alliance between justice and power, Hobbes cannot countenance the division of sovereign power, as seemingly occurs in mixed regimes. Sovereignty, whether it resides in a multitude or in a single person, must be, and in practice is, absolute. But by the same token sovereign power is instituted to serve a specific purpose, and its failure to do so is grounds for revolt. Thus sovereign power is simultaneously accountable to no one and is subject to review and condemnation by everyone. As if to embody this ambiguity, Hobbes fled London at the time of the Long Parliament, Reik informs us, because he feared the consequences of his absolutist view; and a dozen or so years later he fled Paris where the English court was in exile because he feared being construed an enemy of the monarchy.

The radical nature of Hobbes' political philosophy made him a famous but much-decried man in his later years: jurists attacked his theory of indivisible sovereignty; clergymen took umbrage over his portrait of human beings as incorrigibly egoistic; and members of the Royal Society belittled his pugnacious offerings in mathematics and science. Of this reception by Restoration England in general, not to say of the substance of Hobbes' thought, Miriam Reik provides an able, well-written, and oftentimes insightful account.

PATRICK COBY

Kenyon College

Bentham's Theory of the Modern State. By Nancy L. Rosenblum. (Cambridge, Mass.: Harvard University Press, 1978. Pp. xv + 169. $15.00.)

Believing either that confession is good for the soul or that one should make one's biases clear, Nancy Rosenblum begins with a candid admission: she is "by political preference an enthusiast of the modern state." The object of her enthusiasm was "born struggling" and is today "under attack." Its enemies include "parochialism," "Enlightenment universalism and its principal heir, socialism" (pp. 1–2; what, one wonders, of modern state socialism?). She is an "enthusiast" in the older, religious meaning of the term. Upset by "the loss of faith in the state idea," she turns for solace to Jeremy Bentham, who, as "a theorist of the modern state," has "proselytized more fiercely on its behalf" than anyone else. She notes with approval that "Bentham shared and even encouraged" the idea of "reason of state," or "Machiavellism," which she defines as "an enduring political attitude that takes the state as an ideal and accepts the imperatives this ideal generates" (p. 4). This paean to Machiavellism and the modern state brings to mind Henry Kissinger's remark about power being the best aphrodisiac. Combining Kissinger's dictum with Acton's, we might say that power thrills, and absolute power thrills absolutely. This gives us an approximation of Rosenblum's view. She waxes ecstatic about the "thrill of power," "the appetite for intrigue," "the pleasurable calculations of diplomacy"—in short, the manifold satisfactions experienced by those who bear "the proud title [of] political 'insider' " (p. 5).

If one didn't know that this was Rosenblum on Bentham, one might almost think one was reading Kissinger on Metternich (or Kissinger on Kissinger, for that matter): "The mark of modern political practice, he insisted, is absolutism," and no one has given "more thought to the necessity and justification of absolute power . . . or cared more how to organize and exercise it than he." Absolutism requires a "higher rationality" which "ought to govern the actions of governors even though it is bound to conflict with the traditional demands of morality, with rights at positive law, and with the unrestrained inclinations of private men." And since "there is no good without evil, . . . he directed rulers to prefer the lesser evil. . . ." No wonder, then, that "the extent of his democratic sympathies is still debated" (pp. 6–7).

But is this absolutist, authoritarian, Machiavellian figure the real Bentham? What kind of case does Rosenblum make, and how good is it? Ironically, this enthusiast of the modern state—whose political sympathies are anything but Marxist—has, in effect, reiterated and substantiated Marx's reading of Bentham. But where Marx was savage in his criticism (see *Capital,* Vol. I, pp. 609–10), Rosenblum is uncritical in her admiration: Rosenblum's meat is Marx's poison. Both are agreed that Bentham was a theorist of the modern state; that the modern state is a sovereign, comprehensive political entity whose monopoly on the means of coercion is constitutionally justified and accounted just or unjust on the basis of efficiency; and that Bentham's political vision is predicated upon a metapolitical model of the human being as rational calculator. Such individuals will be served best, i.e., most efficiently, by a certain kind of political system, namely, one that is suited to their "nature." And since people are hedonists by nature, they are subject to "two sovereign masters, pain and pleasure." The state serves the latter by having recourse to the former; it promotes the general happiness by threatening to inflict pain upon individual deviants. The law, or more precisely its makers and enforcers, are the means to this end. Armed with a "social psychology" adequately reflecting human nature, the legislator devises laws the overriding aim of which is to secure our expectations (Ch. 2). Good laws are those which, when properly enforced by responsible public officials (Ch. 6), permit us to plan and carry out our private projects. "The state [as] a legal entity" with "its ethical basis [in] individualism" is, as Rosenblum rightly insists, "a peculiarly modern conception of political order" (p. 151). To this view Marx assented, and added: it is a conception of political order that serves the ideological function of justifying the actions of an emerging class of rational self-interested calculators; its conception of the individual and of human nature is the conception which that class has of itself. It is for this reason that Marx referred to Bentham as "that insipid, pedantic, leather-tongued oracle of the ordinary bourgeois intelligence." Unkind but not untrue.

Unlike Marx, however, Rosenblum comes not to bury Bentham but to praise him. Her book is a concise, if uncritical, compendium of Bentham's views on a host of topics relating to the modern state. Concerned as she is to describe and explicate, she is not interested in the sources of Bentham's ideas, nor in exposing contradictions within them, nor yet in answering Bentham's critics. She is content to describe Bentham's views and his reasons for holding them, leaving it up to the reader to judge the cogency, coherence, and adequacy of those reasons. Within these limits I should say that Rosenblum has written an interesting book from which all—even those who do not share her enthusiasm for the modern state—may profit.

TERENCE BALL

Nuffield College, Oxford

Abraham Lincoln and American Political Religion. By Glen E. Thurow. (Albany: State University of New York Press, 1976. Pp. xiii + 133. $10.00.)

A simple truth too often ignored in political philosophy is that a good result depends upon a sound beginning. It makes a considerable difference—perhaps all the difference—whether one approaches political behavior from the position that it can be understood in its own terms, or whether one assumes that it can only be understood as the result of determining events and opinions outside itself. Thurow's analysis of Lincoln is based on the assumptions, once widely held but now seldom adhered to, that (1) a statesman's political opinions and convictions may be known to himself, and (2) may be known to others through public speeches and writings. The elucidation of Lincoln's understanding of the founding principles of this nation, and the relation that his own thought and action bear to them is the subject of this book.

Thurow proceeds by means of a close textual analysis of Lincoln's Temperance and Lyceum speeches, followed by the Gettysburg and Second Inaugural Addresses. His interpretations, especially of the former, are heavily indebted to Harry V. Haffa's *Crisis of the House Divided* (Doubleday, 1959).

For some time now it has been a disputed point whether Lincoln's repeated appeal to the Founders is to be understood as indicating his complete acceptance of their principles or whether, in appealing to those principles, he did not transform as well as invigorate them. In precise and forceful prose Thurow argues that Lincoln did indeed find fault with the Founders. The political and moral crisis that eventuated in the Civil War has its roots in the inadequacies of the Declaration of Independence.

The American Revolution required for its success a rejection of traditional allegiances that culminated in a love of experimentation and novelty. Only such an openness to the unknown could have turned rebellion into revolution. During the Founding period these passions were naturally directed toward establishing the new republic. But revolutionary passions are ill suited to the preservation, as distinguished from the establishment, of republicanism.

The Declaration cannot offer a countervailing force to these passions, Thurow argues, because it is based on a self-evident truth, and truth is notoriously weak when confronted with passion and interest. "Passions can only be controlled by counter passions, not by a mere statement of the truth" (p. 107). Lincoln's moral and geometrical solution was to convert the *truth* of the Declaration into a *proposition* to be demonstrated. Henceforth, "the principle becomes the end or goal towards which the country must work, rather than the beginning from which all other political principles are derived" (p. 76). Thus is created a new passion for human equality or justice, accomplished by "uniting Christianity to the work of the Fathers" (p. 36). The injustice of inequality is now to be understood as a transgression of God's law, to be rectified only by a recommitment to the principle of human equality.

Without denying the force of this argument, one may ask whether Thurow has not gone too far. Is it altogether accurate to say that "although our fathers brought forth a new nation, they turned a child loose whose final form is not determined by them" (p. 80)? Examining the Declaration afresh, would we not find it to be a bit less Lockean than is implied by this statement? Or, to put it another way, could it not be said that the originating principles of this nation contain far more of Lincoln's position than is admitted by Thurow's thesis? And then one must always bear in mind the changing political requirements of statecraft. It may well be that what sound principle required in the period preceding the Civil War differed markedly from what sound principle—the same sound principle—required in 1776.

However this may be, Thurow has written a tightly argued and thought-provoking book. It should be welcomed by anyone interested in Lincoln or the Founding.

JEFFREY D. WALLIN

Arkansas State University

The Tragic Sense of Political Life. By Michael A. Weinstein. (Columbia: University of South Carolina Press, 1977. Pp. x + 189. $9.95.)

Michael Weinstein is an avatar, passing through. He is in agony: a renegade, an anarchist who wears his privilege like a hair-shirt, a rationalist who has stretched his *logos* beyond its conventional tether, has relativized himself through multiple psycho-social self-analyses and has embraced what he calls the Hellenism of our times, marked by the diversity of competing belief systems and alternative histories concomitant with a crisis of imperialism. Here he develops a political philosophy based on the "irreducibility of conflict, the cultivation of ambivalence, and the tolerance, if not the embrace of ambiguity" (p. 16).

Most of his companions are dead: Kierkegaard, Pascal, Nietzsche, Heidegger and Unamuno are in him, grafted onto the American roots of Elijah Jordan and Josiah Royce. In this, his twelfth book, he plays Kierkegaard to Talcott Parsons' Hegel. He views the Parsonian lexicon as a pathetic, banal synthesis of nineteenth-century idealism and positivism, descriptive of life in today's "conglomerates" (superorganizations that unite a number of subunits, performing tasks which may contradict one another, competing to accumulate the means to make history: wealth, power, influence, loyalty). Weinstein argues that the essential tendency of conglomerates is toward totalitarianism, that ours will be remembered (if we survive) as the century of the concentration camp, that Parsons is its most complete theorist, Hobbes institutionalized.

This book is a call to recognize and accept yourself as a crisis and an agony, or in other words, a "person." If you look with pride on your academic "vita," if you confidently proclaim yourself a Marxist or a realist, if you find meaning in concepts like "history, society, nation, state, race, civilization, organization," if you take comfort by defining yourself in terms of such categorical addictions, substitutes for a lost absolute, the old *logos* peddler will try his best to bring you to hour knees: up against the blackboard, infra-man, last man, thinking you have invented happiness, free from guilt and *ressentiment* through behavioral conditioning. Ha! You can't be a person and work in a bureaucracy, and how can you *not* work in a bureaucracy?

Weinstein's "agonic anarchism" is the project of voluntarily committing oneself to appreciating and participating in the oppressive context. Child of the secular city, he digs it. As a "renegade" he stands opposed to all forms of social control (coercion, inducement, flattery and mobilization of guilt). Tenured professor, unwilling to be a saint or martyr, he recognizes that he must use the modes of instrumental social coercion in his rebellion, perhaps even strengthening and reinforcing the organizations against which he rebels, from which there is no exit.

The "tragedy" of contemporary political life is the contradiction between the "will to historical meaning" (like most political scientists Weinstein knows the yearning for world-historical identity, the Henry Kissinger wet-dream of our Machiavellian temptations) and the "relativism which follows from careful examination of one's context through multiple perspectives." In this book, "anarchism" is primarily an epistemological category, the choice of Pascalian lucidity (doubt in order to doubt again) in the face of our dominant Cartesianism (doubt in order to affirm). Look closely, he says, and you will see your will as a lived contradiction. Nobody is immune. We all want some form of "both and," but Weinstein, following Kierkegaard, gives us the existential "either/or: you will regret *both.*"

At the University of California, Berkeley, Edwards Field track stadium is ringed by a barbed-wire fence, looped with razor blades in each circle, fifteen feet to the top of the rectangular stockade. It is not clear whether the fence is there to keep people out, or in. Who can say with confidence whether one teaches in a prison or a school or a factory, or what difference it makes, for what purpose one disciplines and punishes? Marx wrote that if we could hear a human language we would experience it as a plea and a begging. Merleau-Ponty once remarked that the problem during the resistance in France was: how could you be a communist and how could you *not* be a communist? Weinstein asks: how can you *not* be a renegade? Sincerity is the criterion: who has the arrogance to call himself a person?

Hard sayings, no hope or faith here, no consolation. Only what Weinstein calls "ruthless compassion" in the struggle for personhood. An invitation to agony, to hear a human, personal language. How can you hear it? How can you *not* hear it?

J. WIKSE

Shimer College

Terrorism and the Liberal State. By Paul Wilkinson. (New York: John Wiley, 1977. Pp. xiv + 257. $14.95.)

This book is not an abstract analysis or "value-free" study of the phenomenon of modern terrorism and its relationship to the liberal democratic polity. Paul Wilkinson has consciously pared down scholarly paraphernalia in favor of a pithier and more polemical approach. The scholarship is in the near background, but is instrumental rather than essential to Wilkinson's purposes. The chief of these, I think, is to alert liberals in the broad European sense, which includes American "conservatives," to the clear and present dangers of a wide variety of terroristic activities and movements. Within this broad notion of liberalism, Wilkinson wishes to convince tender-minded liberals that the time has come for tough-mindedness about escalating terrorism.

Sympathy with the "wretched of the earth" might dispose tender-minded liberals to excuse, if not to condone, certain terroristic actions. Even if their attitude is wholly condemnatory, the civil libertarianism of tender-minded liberals might cause them to blanch before effective counter-terrorist policies. Wilkinson's tough-minded liberalism hopes to convince the tender-minded that liberalism is a philosophy of state; it is not anarchism. The state accordingly is an organ of social self-defense to be used against those groups who have declared war on civility and the liberal polity. Whatever the ultimate goals of terrorist groups, their operational code that the end justifies the means, any means, involves a war of attrition against the values of liberal civilization. To Wilkinson it is clear that these values are worth fighting for and that liberalism's concern for individual rights is no barrier against serious counter-terrorist measures. There is a long distance between the current (though decreasingly) inadequate policies of liberal regimes and the threat of a police state.

The conception of terrorism that informs Wilkinson's book is this: "Political terrorism may be briefly defined as coercive intimidation. It is the systematic use of murder and destruction, and the threat of murder and destruction in order to terrorize individuals, groups, communities or governments into conceding to the terrorists' political demands. It is one of the oldest techniques of psychological warfare" (p. 49). Wilkinson is at pains to distinguish terrorism, thus construed, from violence, especially legitimate violence, which is inseparable from any state, liberal or otherwise. A liberal polity thus has not only a right but indeed a duty to employ force in the legitimate self-defence of itself and its citizens.

The first half of the book is devoted to establishing the above framework. It involves examining liberalism from a philosophical standpoint and terrorism from a social science perspective. The latter includes a useful summation of the types and roots of terrorism, where the author avoids any simplistic, say, psychoanalytical, account of this phenomenon. The second part of the book reflects a "policy science" commitment whereby Wilkinson goes into considerable detail about a wide array of counter-terrorist strategies, tactics, and devices. Here the concern is combating internal terrorism as with urban guerrillas and "incipient civil war" (e.g., Northern Ireland) and "international terrorism" as with highjacking and diplomatic kidnapping. In either case the author ranges from brief case studies to evaluation and recommendation of proper police and military behavior and techniques. Thus we find him saying, for example, that the "new American 180 laser sub-machine gun should prove an invaluable anti-terrorist weapon" (p. 144).

In sum, Wilkinson's book is a somewhat unpleasant book about a most unpleasant subject. It has all the strengths and all the weaknesses of the "policy science" approach. In its specific concern the book seems measured and realistic. Its polemical zeal stems from the author's conviction that "terrorism is more than simply a manifestation of psychopathology, and more than a symptom of social discontent, oppression, and injustice—though it may be both of these things as well. It is also a moral crime, a crime against humanity, an attack not only on our security, our rule of law, and the safety of the state, but on civilized society itself" (p. 66).

MARK N. HAGOPIAN

American International College

Adam Smith's Politics: An Essay in Historiographic Revision. By Donald Winch. (Cambridge: Cambridge University Press, 1978. Pp. xi + 206. $22.95, cloth; $6.95, paper.)

Donald Winch maintains that the interpretation of Adam Smith's politics has been confused by treating his account of commercial society as an approximation to later conceptions of "liberal capitalism" and missing his central preoccupation with the political problems arising out of the relationships between commercial society and a regime of liberty and justice. For Smith, Winch shows, these are problems to be generally understood by "natural jurisprudence," to be specified by "experimental" and historical thinking, and to be managed by political wisdom. The nineteenth-century theorists with which he is mistakenly associated define the political concepts in terms of the socioeconomic and merge the accounts of primary processes in the two or three domains which Smith distinguishes. To ascribe such views to Smith deprives us of an understanding of his distinctive position and obscures the need to account for the subsequent shift to the later types of theories. Winch has reviewed the textual evidence in the context of political themes promine among Smith's contemporaries and he has uncovered a complex political conception and a distinctive style of political analysis.

According to Winch, Smith's actual intellectual agenda is strongly influenced by the debates about the political implications of commerce begun in the preceding century and carried on in his time between thinkers who have been recently studied as continuators of a republican tradition of civic humanism and others characterized by Duncan Forbes as proponents of "sceptical Whiggism." Like Forbes, Winch generally takes Smith as representing the latter school of thought, which teaches that commercial society fosters habits and relationships which contribute to liberty and justice. At the same time, Winch shows, Smith must address issues raised by those who see in the triumph of commerce a dangerous source of corruption in the civic order, and he identifies some threatening effects they had not seen, chiefly concerning the degradation of the poor through the division of labor and the political machinations of the merchant class. Winch demonstrates that for Smith the political disadvantages resulting from commercial society can only be countered and the political advantages enjoyed by means of an adequate political conception embodied in wise leaders ruling within an appropriate political constitution. The theory of political economy does not itself provide that political conception and Smith never imagined that it could.

Winch expounds his interpretation in a sequence of studies taking up in turn Smith's relationship to the natural jurisprudence of his teacher, Francis Hutcheson, his appreciation of commercial society as a precondition for a political system of constitutional liberty, and his encounters with the great questions of the public debt, the militia, and policy toward America. The treatments, especially in the last three of these chapters, are full of rich and surprising insights. What emerges is the portrait of a political thinker with a far more generous vision of the ends of government than is credited by those who think that he intended it merely to perform necessary services for those in the marketplace. Winch stresses Smith's concern for the welfare and independence of the poor, his regard for the intrinsic moral worth of the governing function, and his detestation for oppressive, arbitrary regimes. Moreover, Smith is shown to have an astute understanding of political structures and processes. His policy studies put the analysis of substantive problems in the context of care for the state of public opinion concerning the authority of government and accommodation to the complex of motives actuating individuals and factions in the governing class. There are no automatic solutions to be expected from the systems comprehended by either of the finished theoretical works. Civilization requires successful interventions by legislators and magistrates exercising the highest form of prudence and benevolence, even while it is constantly jeopardized by the ignorance and ambition of factious politicians.

The book tells all this very well and fulfills the major promise of the title. The uncertain places are those where questions about the philosophical character of Smith's political thought impinge on the political account. One difficulty concerns the vexing questions about the interrelationships between arguments derived from universal theories of human nature or of the natural economic system and arguments derived from the accounts of historical variety and change, and how both sorts of arguments bear on the justification of ethical judgments. Winch disclaims revisionist ambitions in this domain and declares himself satisfied with the view that Smith's historical interpretations guide the "experimental" specification and application of universal principles derived from the theory of human nature. While he is surely right to argue that Smith's major theories do not rest upon a "philosophy of history" in the nineteenth-century continental sense—let alone the economic determinist philosophy of history—Winch brushes past some materials that suggest that the case is more complicated and unresolved than he allows. But Winch seems willing to leave speculative difficulties to others. He provides a subtle and illuminating reading of Smith's political writings and a valuable corrective to anachronistic stereotypes which hamper the study of modern liberal thought.

DAVID KETTLER

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American Politics

Constitutional Language: An Interpretation of Judicial Decision. By John Brigham. (Westport, Conn.: Greenwood Press, 1978. Pp. viii + 182. $15.95.)

This is a good book. It provides aid and comfort neither to behavioralists nor traditionalists. This is not the reason why it is a good book. It is good because the author's perceptive and insightful analysis of constitutional language points up a major shortcoming in the work of scholars in the vineyard of behavioralism as well as that of those who labor in traditional workways. The value of the work of both will be substantially enhanced if Brigham's counsel is heeded.

Behavioralists are faulted, not because of any failure to accomplish what they set out to do, but because they set their sights too low: "Research of this nature, as conventionally operationalized, permits no intermediary position between 'laws' on the one hand and 'men' on the other; . . . the dichotomy evident in this position has inhibited explanation of the legal process. It has driven a wedge between law, the nonfactual norms, and reality, the observable behavior of judges. The behavior of judges, although it tells 'what the law is,' is analytically distinguishable from the body of 'law' " (p. 39). A similar criticism is made of the traditionalists: "The impact of grammar on a decision is far more subtle and potentially more revealing than the rules that we traditionally look to for insight into the role of law in judicial interpretation" (p. 96).

Brigham's thesis is that the language of a judicial decision, rather than rules or attitudes, best portrays the work of the Supreme Court. The limitation that operates is "the authority of symbols rather than the symbols of authority" (p. 161). Brigham documents his thesis by semantic analysis of language and by reference to modern theories of language and linguistics. His treatment of the right to privacy in chapters 6 and 8 is especially telling. By treating the right to privacy—along with other provisions of the Constitution—as examples of the grammar of constitutional law Brigham establishes constitutional law not merely as an activity with a technical vocabulary, but as a grammar dependent on the existence of unique practices. As he points out, "If the process were merely one of looking up precedents, then of course, there would be no difference in the decision-making capabilities and propensities of a layman and a judge trained in the law. The fact that all Justices of the Supreme Court have been trained in the law has meant that the decisions they render are different from those that would be rendered by lay magistrates" (p. 94).

It is unfortunate that the quality of the book's contents has been sullied by sloppy editing and gross overpricing. Not only do typographical errors and misspellings abound—e.g., "philsophers" (p. 146), "porposed" (p. 86), "Herman C. Pritchett" (p. 168), "*Village of Belle Terre* v. *Boraos*" (p. 158)—but also such nonsense as the following: "In the constitutional tradition, a practice is defined by its grammar rather than by the rules which apply to it. Although practice will be describable entirely in terms of their rules. Procedures sense of what these practices are" (p. 97). Garbled passages are sometimes excusable, but to overlook them in a book that emphasizes grammar as heavily as this one does asks too much.

HAROLD J. SPAETH

Michigan State University

The Constitution Between Friends: Congress, the President, and the Law. By Louis Fisher. (New York: St. Martin's Press, 1978. Pp. xii + 274. $12.95, cloth; $5.95, paper.)

Louis Fisher's new book performs a number of valuable services for students of the Congress and the presidency. Though in 1972 he published a book, *President and Congress: Power and Policy* that dealt with institutional theory and practice, his current book focuses more directly and consistently on such issues and also provides an up-dated examination of the significant events of the mid-1970s. In this book Fisher accordingly organizes his discussion around such topics as the appointment and removal powers of the president, presidential and congressional vetoes, congressional investigations and executive privilege, treaties and executive agreements, the appropriations power, administrative lawmaking, and the war power. In each case he treats the historical development of issues and practice as a prelude to delineating the current state of agreement and dispute. He relies heavily on judicial decisions in establishing points of reference in his analysis; but he is also sensitive to the impact of institutional practice and to the political significance of key historical outcomes and current issues. Last but not least, he seeks in important areas of current controversy to define and defend policy standards of his own.

As might be expected in such a comprehensive and ambitious undertaking, the quality of the treatment varies from topic to topic. Though Fisher's knowledge is impressive, he nonetheless delineates issues, practice, and political implications far better in some areas than in others. Moreover, the ad hoc manner in which he defines policy standards tends to limit their depth and persuasiveness. Overall, however, Fisher provides an informative, intelligent, and often perceptive survey of the primary features of institutional terrain laid down in the Constitution.

Yet the significance of this book, and of Fisher's work generally, is broader than the substantive content of his analysis. His avowed aim is to reassert the centrality of formal powers and prerogatives as an aspect of congressional and presidential politics and to encourage political scientists to engage in institutional policy debates (p. vii). Fisher thus represents the continuation of a long tradition in American political science—the institutional, public law tradition. This tradition, to be sure, has not been dominant among students of the Congress and presidency since the advent of the behavioral revolution in the 1950s. Indeed, in the early years of the behavioral movement many of its advocates attacked the older tradition with fervor. They argued, not without reason in the context of much of the existing literature, that for political science to advance as a discipline, the focus of research should be on individual behavior and process, that formal powers and prerogatives were of secondary, if not superficial, importance, and that the province of the political scientist could not encompass evaluation. Ironically enough, however, the 1970s have witnessed a new receptivity to traditional institutionalist premises owing both to the character of institutional conflict and change and to increased awareness of the limitations of prevailing modes of behavioral research. Aside from contributing to our knowledge, Fisher's new book thus also has the virtue of incorporating traditional institutionalist assumptions regarding context, structure, and reason, and by so doing reminding us of the continuing need to take them into account. Put in more modern garb, these assumptions may be summarized as follows:

(1) In the case of complex organizations the wholes are always more than the sum of the parts and environmental relationships and interdependencies are of critical significance. Treatment of the Congress and the presidency as wholes in rapidly changing and uncertain environments is thus critical to an understanding of these entities both in terms of internal structures and external relationships.

(2) Internal structures and external relationships involve normatively patterned interaction, some critical portions of which are formalized in law and rules. At any point in time these formalized patterns and the shared conceptions of norms underlying them constrain and affect the distribution of influence, the character of behavior, and the determination of substantive outcomes. Moreover, some significant aspects of such patterns and norms are continually in flux as a consequence of changing environmental pressures. Hence, a critical dimension of the politics of the Congress and the presidency relates to conflicts and outcomes in these regards.

(3) Rational persuasion on the basis of shared values as well as bargaining on the basis of interest plays a role in such conflicts. Ideas are indeed weapons, but they are not equal in weight nor open to unconstrained manipulation in the service of interest. Therefore, not only should reductionism in terms of reason or interest be avoided; in addition, students of politics can and should contribute to institutional policy debates. The arena should not be left to lawyers and historians who are less apt to take basic political factors or implications into account.

It is no criticism of Fisher to note that he neither attempts nor accomplishes any consistent or far-reaching integration of context, structure, and behavior. He well understands the need to treat the Congress and presidency as wholes, the political significance of formalized relationships, and the place of rational persuasion in institutional policy debates. But as in the case of the best of the pre-1950 literature, the theoretical apparatus employed lacks the concepts and measures needed to reduce the elements of vagueness and imprecision that persist in institutionalist analysis. As for more empirically oriented students, they have not as yet provided much assistance in this regard. Despite a growing realization of the need to deal explicitly with context and structure as critical variables, analysts in the behavioral tradition remain reluctant to approach the Congress and presidency as wholes—to focus on their roles or positions, interdependencies, and conflicts. The importance of context is far more widely recognized than formerly, but still largely with reference to individual behavior and either in terms of highly discrete aspects of internal structure or broad single-factor theories which are then accorded all-encompassing explanatory significance. Here too, then, much remains to be done, but the prospects for synthesizing the best of the institutionalist and behavioral traditions are brighter than at any time in the past.

JOSEPH COOPER

Rice University

Democracy and the Amendments to the Constitution. By Alan P. Grimes. (Lexington, Mass.: Lexington Books, 1978. Pp. xi + 190. $17.95.)

The "constitutional road to the decision of the people," admonished Madison in the 49th *Federalist,* should be "kept open for great and extraordinary occasions." Granted that not all constitutional amendments have met the Founders' stringent standards, we might nonetheless be disconcerted by the assertion that amendments to the Constitution of the United States are "public evidences of a transfer of power into the hands of a new majority, . . . settlements that the dominant party or coalition perceives to be advantageous to their continuance in power" (p. 162), and are a means by which "the power of an opposing elite" has been "undermined" (p. 163). Grimes could be right, of course. The motivations for and the processes of constitutional change may indeed be equated with garden-variety politicking, bearing little relationship to the serious concerns underlying Article 5. But we are entitled to some forthright discussion on the point.

This criticism aside, there is no doubt that Grimes has provided a fresh, ingenious, engaging and most useful perspective on the objectives, politics and results of constitutional revision. Carefully documenting his case, he argues that the overall "thrust" of "amendment politics" has been in the direction of "equalitarian democracy," defined as a system of "equal rights and majority rule" (p. 26), and that most amendments reflect sectional and/or demographic interests, ideologies and sentiments.

The first 12, which guarantee individual rights and limit the power of the federal government (1–9), provide for a degree of state autonomy (10 and 11), and (by assuring that the president and vice-president are of the same political persuasion) bolster the principle of majority rule (12), he classifies as southern; and the abolitionist-inspired 13th, 14th, and 15th as northern amendments. Their Populist/Progressive origins designate 16–19 (income tax, direct election of senators, prohibition, women's suffrage) as western; and the 23rd through the proposed 27th (partial enfranchisement for residents of the District of Columbia, abolition of the poll tax, presidential succession and disability, enfranchisement for 18-year-olds, women's rights) are viewed as urban amendments. Amendments 20, 21, 22 and the unratified Child Labor Amendment ("lame duck," repeal of prohibition, presidential two term limitation) adopted during the period in which the politics of urban America supplanted the politics of regional and rural America, are tagged transitional amendments. Here Grimes is especially resourceful, arguing that the 20th, usually considered along with the 12th to be a technical or corrective amendment, has had, by shortening the period between election and assumption of office, the same effect of operationalizing the principle of majority rule. Some amendments, 11, 18, 21, 22, and 25, do not of course fit neatly into their assigned slots and have not furthered egalitarianism and/or majoritarianism. Grimes deals adequately with what are not awkward exceptions.

In addition to the classification system, the value of the book is enhanced by the way Grimes opens our eyes to the relationship between the adoption of amendments in clusters and the rise of new power blocs and constituencies. Amendments 1–12, ratified between 1791 and 1804, "signaled the triumph of the Jeffersonian[s], . . . over the Federalists" (p. 158). With the next three (1865–1870) the northern Republicans "sought to check the political power of the heirs of Jefferson and Jackson" (p. 158). The Western (1913–1920), Transitional (1933 and 1947, the sole exception to the clustering effect) and the Urban (1961–1970) Amendments in turn "rewrote the constitutional rules" (p. 159) in order to make significant inroads into the power of the Republican party and the southern establishment.

Whatever reservations may persist about the perhaps too facile "amendment politics" approach are compensated for by Grimes' illuminating analysis which forces us, like it or not, to reorient our thinking about constitutional history. And because his book is so succinct and clearly written, so well organized, informative and often lively, it is strongly recommended for undergraduates—not only for their edification, but for their enjoyment as well.

MARY CORNELIA PORTER

Barat College

The New Politics of Food. Edited by Don F. Hadwiger and William P. Browne. (Lexington, Mass.: Lexington Books, 1978. Pp. ix + 267. $19.95.)

The 1973 Agriculture Act was supposed to mark the end of the era in which the federal government intervened energetically to raise farm prices and incomes. Henceforth, farmers were to make their living from the market, with federal subsidies provided only during rare disasters. The future for farmers looked bright as massive wheat sales to the Soviet Union helped produce a sharp upturn in farm prices. Yet in 1977, Congress passed another Agriculture Act to raise farm prices through government intervention. Prompted partly by a new, militant group, the American Agriculture Movement, Congress was so willing to return to agricultural subsidies that vigorous efforts by the White House were necessary to limit the extent of the federal government's commitment to acceptable levels. Political scientists may be inclined to mutter *"plus ça change,"* and pass on.

The contributors to this collection would argue that such a reaction is simplistic. While, as Browne notes in his introduction, "the authors seem to agree that the agricultural subsystem as a whole is alive and well" (p. 3), much too has changed. The grassroots rebellion which produced the American Agriculture Movement (which receives regrettably little attention in this collection) has had its effect not only on politicians, but also on existing farm organizations; apparently even the American Farm Bureau Federation was forced to modify its extreme laissez-faire views. But more significantly, new actors have forced their way onto the stage. Agricultural policy making is no longer just for farmers and their representatives. Environmentalists have raised subversive questions about current farming techniques and the direction in which research is taking the industry. Representatives of the consumer groups have succeeded in winning appointments under the Carter administration even in the Department of Agriculture itself. In a period when inflation and the level of government expenditure have become popular political issues, it is less easy to assume that either the consumer or the government will pick up the tab unquestioningly for farm subsidy programs. The steady decline in the number of full-time farmers in the U.S. scarcely makes easier the task of creating a political coalition to defend farm subsidies.

The contributors to this study had a most valuable opportunity to describe and explain the changes in agricultural politics which these changes have produced. Yet the collection fails to exploit this opportunity and indeed notably lacks coherence. Many of the chapters do contain fresh information or ideas which political scientists will find useful. Thus Browne and Wiggins provide an interesting discussion of the general farm organizations. Berry contributes a useful account of developments in the food stamp program and two contributors make praiseworthy attempts to test aspects of the well-worn clientele theory (which their findings do not substantiate). But few common themes or questions link the chapters together: the attempt to monitor changes in the politics of food is soon forgotten. Major aspects of the current politics of food are neglected; thus no contributor discusses the possibly enhanced role of commodity pressure groups, and the chapter on the executive and agricultural politics is merely four and a half pages long. But above all, the contributors lack any adequate picture of the old agricultural politics with which current practices might be compared systematically. Insofar as the contributors implicitly share a picture of past agricultural politics, it is that of the "iron triangle" and clientelism, usually substantiated by references to general works on American government. Little account is taken of the implications of the analyses of writers such as Mayhew and Clausen on congressional support for farm programs. Indeed, it is regrettable that the chapters on the congressional politics of the 1977 Agriculture Act make so few comparisons with voting patterns on agriculture acts in the 1950s and 1960s. One would like to know whether the findings of older studies by Mayhew, Clausen et al. about the relative importance of constituency, ideology and party still stand. Much the same can be said, *mutatis mutandis,* of chapters on other aspects of the policy process. One wonders whether the explanation for such omissions is that contributors were given too little space to develop their ideas; a text of less than 200 pages is divided into 21 chapters. Perhaps fewer but longer contributions would have produced a collection which explored more systematically the new politics of food.

GRAHAM K. WILSON

University of Essex

Small and Large Together: Governing the Metropolis. By Howard W. Hallman. (Beverly Hills, Calif.: Sage Library of Social Research, Vol. 56, 1977. Pp. 288. $14.00, cloth; $6.95, paper.)

This book will prove useful to scholars, students, reformers, and politicians. Scholars who have not yet read the 16 reports of the Advisory Commission on Intergovernmental Relations or studied the 276 Standard Metropolitan Statistical Areas (SMSAs) of the U.S. will appreciate the complexity of the metropolitan areas described in *Small and Large Together*. Students will find this a good introduction to the problems and prospects of government in the metropolitan areas where 73.2 percent of all Americans live. Would-be reformers and politicians will be introduced to competing political philosophies underlying many of the alternative proposals for governing these metropolitan areas.

The principal strength of the book is its treatment of the complexity and diversity of the 276 different SMSAs. While advocating local federalism as a general cure for dysfunctional aspects of current local governments in metropolitan areas, Hallman recognizes that this will take different forms depending upon the size of the population, the number of counties, and different existing political and governmental patterns in each area.

Despite his full treatment of complexity and diversity, Hallman advocates a specific point of view with which he concludes the book:

> Metropolitan society is pluralistic, so its governmental structure should be diversified. But not so fragmented as to be incoherent and ineffective. As metropolitan citizens we need an areawide general-purpose government responsible to us through direct elections; as neighborhood citizens we need a governmental unit closer to home; and as citizens of intermediate areas—city or county—we need this focus too. All this is possible under local federalism, the system which brings small and large together into a sensible whole (p. 268).

The merit of Hallman's prescription is his attempt to join the movement for neighborhood government which seeks to bring government within the reach of each citizen, with the movement which seeks to fashion a government for the emerging metropolitan community with sufficient scope and powers to govern these hugh areas effectively. Hallman's local federalism allows the flexibility not only to join together both larger and smaller units of government but to do so without radically changing existing governments which have the power to block change. For many readers Hallman's local federalism will prove a touchstone by which to refashion current local government in metropolitan areas.

Hallman's book, with its mix of case studies, general statistics on all SMSAs, and competing political theories, overcomes the limitations of case studies of single metropolitan areas or single political philosophies. It suffers, however, from three limitations of its own. First, it does not "prove" that local federalism is inevitable or even the most likely governmental structure for the metropolitan area—it is a hope for reform rather than a compilation of provable hypotheses. Second, it does not erect a firm philosophical foundation for local federalism; rather, in the American reform tradition, it proposes a pragmatic solution which is grounded neither in assertions about the nature of man nor about the ideal society to be achieved. Finally, it does not provide a strategic analysis of the political basis for the proposed change, although trends and tactics are discussed. Nonetheless, Hallman provides a solidly researched book which captures the complexity of metropolitan areas and their governments, a useful book for scholars, students, and practitioners, and a hopeful book which advocates better government for the three-fourths of all Americans who live in urban areas.

DICK SIMPSON

University of Illinois, Chicago Circle

The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority. By Thomas L. Haskell. (Urbana: University of Illinois Press, 1977. Pp. xii + 276. $12.00.)

The creation, development and death of the American Social Science Association were integral to the sweeping changes in the educational, scientific and professional life of the latter part of the nineteenth century and the beginning of the twentieth. New university curricula, new graduate programs, new scientific societies, new professional organizations were the order of the day. At the same time, many of the leaders in these developments were greatly concerned about public policy and actively interested in public affairs. It was such people—both interested in science and in public policy—who created the American Social Science Association.

Concern about support for development of the natural sciences led to the creation of the American Association for the Advancement of Science (1847) and the National Academy of Sciences (1867). Concern about developing the social sciences and applying social scientific knowledge to the public problems of the day led to the creation of the American Social Science Association (1865) and the American Academy of Political and Social Science (1890). Concern about the need for specialization and professionalization of the individual disciplines led to the creation of the American Historical Association (1885), the American Economics Association (1885), the American Political Science Association (1903), and the American Sociological Association (1905). Concern about all these matters led to the new university curricula and the new graduate programs such as those initiated by John W. Burgess at Columbia (the School of Political Science was created in 1880), by President Andrew D. White at Cornell and President Daniel C. Gilman at Johns Hopkins.

Thomas L. Haskell's interesting and significant book brings us to one of the centers of such activity, the American Social Science Association, organized in 1865. The central focus of Haskell's book is the rise and fall of that organization. Most of the leaders in the intellectual and professional life of the times were involved: Daniel Gilman, Andrew White, William Lloyd Garrison, Thomas Hill, Theodore Douglas Woolsey, Frank Sanborn, William Barton Rogers, A. B. Palmer, Samuel Gridley Howe, Simeon Baldwin and Edward Jarvis, to name only a few. Haskell's story is well told and includes in the footnotes and bibliography useful sources for those who wish to explore the intellectual developments of the latter half of the nineteenth century and the beginning of the twentieth.

Haskell notes, and the history of the period confirms, that major changes took place in intellectual and educational life between the founding of the American Social Science Association in 1865 and its death in 1909. For example, when the American Social Science Association was founded in 1865, there were virtually "no specialized professional social scientists in this country or, strictly speaking, in any other." But when the ASSA died in 1909, each of the specialized sciences were sufficiently developed to have created separate professional organizations, separate journals, and annual meetings—all based on newly developed specialized curricula at the university level. The ASSA played a significant role in these developments. Yet, at the same time, it was centrally concerned with political, social and economic reform, with solving the social problems of the day. There was continual tension between science and reform and this plus the lack of a solid basis in the academic disciplines in the universities accounted, in part, for the ASSA's failure to survive.

The internal conflict between reform and science is one of the most interesting parts of Haskell's story. In an era that was moving rapidly toward specialization and professionalization and that carried with it a strong element of positivism, the leading figure in the founding and development of the ASSA was Frank Sanborn, more reformer than scientist. He was secretary of the ASSA for 33 years and devoted more time and attention to the organization than any other individual. Sanborn believed that the term "social science" was "a convenient rubric for a kind of inquiry and reform activity in which scientists had no edge over novelists"; he characterized the *Journal of Social Science* as "my serial novel." He was, as Haskell reports, "free lance intellectual, teacher, poet, journalist, radical firebrand, polite reformer, government bureaucrat, philosopher, classicist, propagandist and philanthropist." In his support of John Brown, he made no pretense of being neutral. "He withheld facts and bent them when necessary to further a goal he felt higher than objectivity." It is easy to recognize in Sanborn a type as well known in the sixties and seventies of this century as in the same decades of the nineteenth.

Yet the ASSA and Sanborn were concerned with professionalization. Gilman of Hopkins, Eliot of Harvard, White of Cornell, Baldwin, founder of the American Bar Association and in 1911 president of the APSA, Benjamin Pierce, Louis Agassiz, Edmund Jones and the Lazzaroni were all active ASSA participants. Side by side, sometimes in the same man, there was strong desire to create the community of the competent and an equally strong desire to contribute to remedying society's ills.

The ASSA was a combination of reformers and scientists: scientists who wished through specialization and professionalization to establish authority based on science, and reformers who wished to use the results of science to prevent crime, treat the insane more humanely, improve sanitation, develop better education, provide relief and reduce unemployment. It was an uneasy combination—not unknown today—of those who primarily wished to understand society and those who primarily wished to improve it. There often was little distinction between scientific inquiry and reformist activism. Unhappily, there was too often an acceptance of the simplistic assumption, by both sides, that all of society's ills would disappear if only the right information could be gathered and rationally analyzed. Provide the facts, it was thought, and solutions will be obvious.

Unfortunately for the ASSA, it did not adjust to the changes taking place. While the ASSA aided in the organization of the new professional associations—such as AHA, AEA, APSA and ASA—it did not recognize that their development sounded the death knell of the ASSA. Daniel C. Gilman, who was invited to make the ASSA a part of Johns Hopkins, had a clearer vision of what was happening when he turned down the proposed merger.

Political scientists interested in the development of the social sciences will find Haskell's story a valuable companion to Somit and Tanenhaus, *The Development of American Political Science.* Haskell's book not only tells us a great deal about our past but also, like all good history, tells us a great deal about our present.

A few final comments: Haskell is interested not only in the history of the ASSA and the years it spanned, but also in an explanation of what happened. This leads him to devote a large part of the introduction to a discussion of the views of Morton White, Talcott Parsons, and H. Stuart Hughes. The discussion is interesting, often insightful, but fundamentally irrelevant to the story of the ASSA and what it tells us about "the crystallization of the social sciences in their present form as academic disciplines" or "the corresponding rise of the professional social scientist as paramount authority on the nature of man and society."

Further, Haskell's continuing effort to explain the development of separate academic disciplines, growing professionalization, and the concurrent rise of the professional social scientist on the basis of the growing "seamless web of interdependence" in society is interesting but unconvincing. In fact, one may justifiably take the reverse view; certainly one may ask which causes which and not come up with Haskell's answer.

Finally, Haskell does not deal effectively with the nineteenth-century "crisis of authority" that, given his title and his expressed concern, is intended to be a central issue of the book. Of course, he was not required to introduce or deal with this problem; his book has great merit without it.

EVRON M. KIRKPATRICK

American Political Science Association

Jefferson and the Presidency: Leadership in the Young Republic. By Robert M. Johnstone, Jr. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 332. $15.00.)

Whether as philosopher or president, Thomas Jefferson is a figure of tantalizingly elusive brilliance who remains, despite a lengthy parade of scholarship, a towering presence, whose many nuances of thought and activity cannot be altogether explained. In this book of thorough scholarship and bold, but measured, interpretation, Robert M. Johnstone, Jr. addresses himself to Jefferson's presidency and the illumination it provides of leadership in a new nation. To structure his study, Johnstone utilizes the analytical design of Richard Neustadt's classic *Presidential Power,* with its emphasis on the president as a persuader and bargainer.

Jefferson fits readily into the Neustadt pattern. By nature, Jefferson was not given to issuing commands to perform the presidential function. Bargaining and the many subtleties of persuasion suited far better his preference to remove the "monocratic" features of the executive, and the reality that the principal arena for policy making was Congress where the presidency was weakest. Jefferson's extensive use of the Republican party, its caucus, and congressional and executive lieutenants was essentially an endeavor in bargaining and persuasion.

As Johnstone makes clear, Jefferson's conduct of the presidency was shaped by four basic assumptions about the purpose of leadership. Above all, in a republic, a leader must be attentive and responsive to the popular will. Although nowadays this is a cliche of textbook civics, it was an idea struggling for status in Jefferson's day. As leader, he did not seek to respond automatically to popular pressures, but to address the more important popular desires and aspirations.

According to a related assumption, the leader must be able to anticipate the popular will, to shape rather than be shaped by it, to educate the people to discern their own best interests. Jefferson also assumed that leadership was instrumental, that its function was to formulate and effectuate programs to make the institutions of government more responsive to popular will. In this sense, leadership was substantive and pragmatic. Finally, Jefferson assumed that the leader must be capable of adapting to circumstance, of growing in office, adjusting or abandoning outmoded views, and of escaping enchainment by outworn dogmas. In essence, Jefferson meant to employ the resources of leadership to bring the nation's political institutions into resonance with its experiment in self-government.

The degree to which Jefferson turned the presidency away from the inherited monarchist habits of the Federalists, and committed it to advancing the popular movement which he headed to return to "first principles" of popular government and political liberty, was astutely recognized by John Quincy Adams: "The power of the [Jefferson] administration rests upon the support of a much stronger majority of the people throughout the Union than the former administrations ever possessed since the first establishment of the Constitution. Whatever the merits or demerits of the former administrations may have been, there never was a system of measures more completely and irrevocably abandoned and rejected by the popular vote" (p. 311).

There were other strings to Jefferson's presidential bow. Though deferential to popular will, he used power boldly in that most autonomous domain of the presidency, foreign affairs. The centerpiece of his success was the Louisiana Purchase. Jefferson's management of it demonstrated that he was not so wedded to constitutional purity that he would let pass an enormous national opportunity. To Madison he wrote, "I infer that the less we say about constitutional difficulties respecting Louisiana the better, and that what is necessary for surmounting them must be done *sub silento*" (p. 72). The key to success was Jefferson's control over Congress, which he needed to exert only moderately because Congress and public opinion readily perceived the merit of the purchase.

As the Louisiana episode suggests, Jefferson perceived the presidency to be the depository of prerogative, the need of the responsible executive to exceed constitutional limits if the public good demanded it. Admittedly, this is one of the more perilous passages to be traversed by the presidency and the nation, a strain on the leader's integrity and his capacity to distinguish resolutely between acts supportive of and acts detrimental to the public good, especially when the actor is also the judge of his own cause. Jefferson always seemed confident of his capacity to discern the public good, spurred by his commitment to popular rule and his supposition that his acts must pass scrutiny at the bar of public accountability.

Paradoxically, for all of Jefferson's regard for popular opinion, he had available only the most primitive tools for ascertaining it. He rejected one of the more established methods of the day, the "swing around the circle" as offensive to republican simplicity and tinged with monarchy. Jefferson chiefly relied on the party press and the exercise of extraordinary diligence in gathering political intelligence. He maintained an enormous personal correspondence with friends and political acquaintances throughout the country, most of it written in his own hand, and exhorting the recipients to write regularly about affairs of state.

Johnstone's volume will stand at the front rank of the huge corpus of writings on Jefferson. With admirable scholarship, a lucid style, and convincing judgment, it deftly challenges several prevailing interpretations of Jefferson and his presidency, and carves out new territory of its own.

LOUIS W. KOENIG

New York University

The Making of Political Women: A Study of Socialization and Role Conflict. By Rita Mae Kelly and Mary Boutilier. (Chicago: Nelson-Hall, 1978. Pp. x + 368. $16.95.)

Students of women in politics appreciate that scholarly wisdom and popular opinion have often presented them with unrealistic perceptions and that analytic models have been sex-biased. Rita Mae Kelly and Mary Boutilier begin to remedy these difficulties in a simultaneously innovative and conventional study of selected political women, socialization factors and conflicted expectations. They provide the basis for a "great woman" theory of comparative politics, when many scholars in this field are moving to social psychological and sociological constructs.

Kelly and Boutilier rely on an eclectic "theoretical" framework constructed from choices made among competing views in a wide range of substantive areas. Their choices often seem rather arbitrary. For example, selecting a linear model of childhood socialization is not uncontroversial. The authors cite the impact of later situational factors (e.g., at pp. 224, 277, 279, 307), but downplay it for their own theoretical requirements (see p. 312). Yet the impact of situational factors as intervening variables probably is quite potent.

The study is conducted within a complex 18-celled typology; the baselines are: (1) sex role ideology, (2) extent and control of life space (one and two combined yield six subdimensions) and (3) nature of political salience (which is trichotomized). The proposed typology is applied to three categories of women: private (i.e., some political wives), public (i.e., issue-oriented political volunteers) and achieving (i.e., well-known political office holders or revolutionaries). The authors find these women types form a continuum for adult political behavior and for the stages of socialization they have traversed. They suggest, however, that "continued social science concentration on the 'typical' and the 'representative' (male or female) and the leaving of the atypical and unrepresentative . . . has meant that the social system always appears to outstrip our ability to understand it" (p. 5). Apparently they mean social science has focused on women as typically apolitical mothers and that they intend to typify more atypical political women, among them terrorists and revolutionaries.

The data, forthcoming from a series of brief biographies, both test and ground the theory. This procedure results in rather disconcerting shifts from deduction to induction. The authors are aware of this. They helpfully base some of their inferences on Guttman scaling, though Guttman scaling is not a powerful analytic technique. The authors must assume, for example, a relationship between independent mothers and achieving political women (see p. 241).

Still, a deductive/inductive approach may be promising in this area, given the premise that past work is suspect. If neither reliable theory nor sound basic research is available for guidance, then both must be generated. Such a dual approach requires interdisciplinary, i.e., historical, skills. Absent these, we may find ourselves saddled with meaningless results. Not that Kelly's and Boutilier's findings are meaningless. Far from it. Their results are, however, subject to rigorous reexamination, given several problems.

First, the biographies, even granting space limitations, are shallow and sometimes stereotypic: for example, the assertion that Elivera Doud, Mamie Eisenhower's mother, "never worked" (p. 215). The authors draw on limited sources: for example, many materials on First Ladies' public interests are ignored. Also, an argument could be made that the proposed categories are not mutually exclusive (but see p. 313). Eleanor Roosevelt is a prime example. The literature is beginning to realize her extensive political participation and its ramifications for the Roosevelt presidency and afterward. There also is some thoughtful research, i.e., in speech communications, which establishes the depth of her self-doubt and ongoing willingness to serve as Franklin Roosevelt's conduit. Abraham Maslow found her self-actualizing; Truman eulogized her as First Lady of the World. This matter may be debated forever; however, it suggests slippage between the private, public and achieving women types.

Further, measurement problems increase with hindsight in this kind of study. The question remains: how to evaluate observations. The book notes Eleanor Roosevelt's rejection of her mother as a role model. What is not noted is Joseph Lash's assertion that Eleanor Roosevelt later understood and appreciated the pressures on, and responsibilities carried by, her mother. Or that Eleanor Roosevelt's maternal grandmother, four aunts and nurse presented models of strong, often unconventional women, before Mlle. Souvestre entered her life. Or that Eleanor Roosevelt's parents both were fatherless and were reared by mothers who had to cope with life directly.

A study positing the importance of early childhood years necessarily depends upon data from that period. It is difficult to attain, even for famous individuals. Kelly and Boutilier frequently confront this problem, for example, in the cases of Mary Wilson and Charlotte Corday. One also encounters "myths" in the case of famous people. For example, the authors note that Maria Tambussi, Ella Grasso's Italian, Roman Catholic mother, appeared a traditional private woman, but that a questionnaire completed by Grasso indicated something else. The authors did not have access to such questionnaires for all their subjects. The authors were not irresponsible in their coding procedures. This is a limitation intrinsic to their data.

Another problem is that Kelly and Boutilier sometimes discuss mostly mainstream literature with relatively few sentences devoted to their own findings, for example, regarding the impact of standard SES variables on female leadership behavior. The authors postulate that using such variables is not particularly fruitful. More than that, they *seem* to be able to support the claim empirically, but further discussion of their findings would have been necessary to do so. They also have a serious problem in their "focused sample" size (n = 36), e.g., in comparing mean educational attainment (pp. 288–89).

Despite these problems, Kelly and Boutilier usefully propose hypotheses and provide a comparative perspective. They do this with a sense of humor and within a nicely structured study. They state "that even with the limitations in the design our efforts are more likely to reveal the processes actually needed to socialize real political women than studies done on random, representative samples of . . . students or even of women in general which may well meet more adequately the rigorous scientific requirements usually established for such studies" (pp. 92–93). They may well be correct. Their study of socialization and role conflict is a landmark effort.

SARAH SLAVIN SCHRAMM

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Presidential Impeachment. By John R. Labovitz. (New Haven, Conn.: Yale University Press, 1978. Pp. xiii + 268. $15.00.)

To the Framers of the Constitution impeachment was an essential safeguard of civil government—a weapon of last resort in defense of liberty, a redress for power abused and trust betrayed, an alternative to insurrection and tyrannicide. What the veto was to the executive—protection from encroachments upon the separation of powers—impeachment was to the legislature. "No point is of more importance," as George Mason put it, "than . . . the right of impeachment."

In their view of impeachment, the Framers were inspired by British example, schooled by British mentors. The epic struggles between liberty and prerogative—struggles in which Parliament had used impeachment as a "bridle on the king"—were regarded by Americans then as part of their own constitutional experience. Books and pamphlets from the mother country—histories which dwelt on the "delinquencies" of Richard II and of the Stuarts, discourses on popular rights and liberties, works of constitutional exegesis, documentary collections and the like—almost all described or explained, invariably eulogized, impeachment. In the colonial era, Americans tried on several occasions to remove officials by impeachment—as late as 1774 in Massachusetts where the house of representatives accused the chief justice of "high crimes and misdemeanors"—but encountered opposition from the Crown. The renunciation of George III by the Continental Congress, in which most of the Framers had served at one time or another, was in form and effect an impeachment. Prior to the Convention in Philadelphia, constitution-makers established the removal process in every new government created for the original states. De Toqueville, commenting on impeachment in both state and federal systems, was much impressed. "There is no question on which the American constitutions agree more fully."

The Framers wrought the presidency in the image of British kingship but denounced the ancient maxim that "the king can do no wrong." They provided expressly for the impeachment of their republican surrogate. This king-sized substitute for regal power *could* do wrong. He would not be shielded from personal responsibility by constitutional fictions or ministerial scapegoats. There would be no legalistic quibbling, much less bloodspilling—as for centuries there had been in Britain—over whether or not authority existed in the Constitution to depose the chief executive. The Framers intended impeachment as a check primarily on the one person entrusted by the Constitution with executive power.

Despite its importance to the American founders, impeachment has prompted few substantial works of scholarship, none by political scientists. Among the few, the productions of lawyers and historians, *Presidential Impeachment* by John R. Labovitz ranks with the best. Labovitz, incidentally, is critical of political scientists for slighting the subject. A lawyer, he served in 1974 on the impeachment inquiry staff of the House Judiciary Committee. Drawing upon the investigation of Richard Nixon's conduct as chief executive, Labovitz presents an informed, cogent analysis of constitutional issues involved in presidential impeachment. His central concern, as unavoidably it must be for any analyst of American impeachment, is the nature of "high crimes and misdemeanors."

Appropriately, the author begins with, and frequently recurs to, the Framers. His explication of their intent is the most satisfactory one now available. Still, in my judgment—I am currently engaged, with support from the National Endowment for the Humanities, in a study of the Framers' intent—Labovitz could have documented his analysis more impressively had he investigated more thoroughly the Framers' views, experiences, sources and authorities. The Framers knew, said and did more about impeachment than is divulged by the few sources Labovitz relies upon—chiefly Farrand, Elliot, *The Federalist*, the *Annals*. These sources do not impart, for example, views expressed by a number of the Framers as they participated in the formulation of state impeachment provisions or were involved personally in state impeachment cases.

To elucidate "high crimes and misdemeanors," Labovitz analyzes the two major impeachments since 1787, those of Justice Samuel Chase and of President Andrew Johnson, which generated and sustained the protracted dispute—Labovitz calls it "one of the longest-running . . . sideshows in American constitutional law" (p. 126)—over the meaning of those words. Chase's defense brought to the fore the newfangled notion that "high crimes and misdemeanors" are indictable offenses, a notion which bedeviled American impeachment thereafter. Had the House of Representatives not been constrained by this notion, Labovitz suggests, the case against Johnson, whose impeachment the author believes may have been justified, might have succeeded. The case should have been built, he argues, principally on constitutional rather than statutory grounds. This contention reflects the author's dominant and pervasive theme.

"High crimes and misdemeanors," Labovitz maintains, are offenses against the Constitution. They are breaches of trust, derelictions of duty, abuses of power. They may or may not involve indictable crimes. Presidential impeachment is justified by persistent failure to "take Care that the Laws be faithfully executed," by violation of the oath "faithfully [to] execute the Office . . . and . . . [to] preserve, protect and defend the Constitution," by egregious misuse of constitutional authority confided to the president's discretion. Infidelity to the Constitution—not indictability—is the essence of "high crimes and misdemeanors." This construction, the view generally of previous commentators, is set forth more effectively by Labovitz than by others.

Again, however, Labovitz could have reinforced his position with fuller documentation, particularly from British writers and precedents familiar to the Framers. These sources make explicit, for example, that impeachment from the beginning was grounded upon standards and conventions distinct from the common law and statutes. This, in an oft-cited precedent, the House of Lords pronounced emphatically in the fourteenth-century reign of Richard II. During the seventeenth and eighteenth centuries, even in the nineteenth, Coke was quoted incessantly, more than any authority in the annals of impeachment, on the *lex et consuetudo parliamenti.* "As every court of justice hath laws and customs for its directions, some by the common law, some by the civil and canon law, some by peculiar laws and customs, &c. so the high court of parliament . . . has its own proper laws and customs." In all the voluminous writings and records on British impeachment available to the Framers, the indictable-crime notion is nowhere mentioned, much less discussed.

Had Labovitz drawn more deeply from these sources, his treatment would have been more decisive not only of "high crimes and misdemeanors" but of other issues too—executive privilege and congressional inquiry, presidential responsibility for subordinates, justiciability and other matters. Even so, his analysis throughout is astute and persuasive. What he has written considerably enriches the literature on impeachment.

MAURICE KLAIN

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The Quest for Justice: The Politics of School Finance Reform. By Richard Lehne. (New York: Longman, 1978. Pp. vi + 246. $11.95, cloth; $4.95, paper.)

A *Time Magazine*-like description of events surrounding a series of New Jersey Supreme Court decisions in *Robinson* v. *Cahill* (62 NJ 473, 1973; 63 NJ 196, 1973; 67 NJ 35, 1975; 67 NJ 333, 1975; 69 NJ 133, 1976; and 70 NJ 155, 1976) almost distracts the reader from noticing the contribution to theory contained in this book. On one level *The Quest for Justice* may be read as an interesting case study of the judicial process, of educational policy, or of public finance. At another level, however, it tests Jonathan Casper's hypothesis, from his *Lawyers Before the Warren Court,* that litigation is a means of access to the policy formation process. Using data from an Eagleton Institute project (92 elite interviews during 1975 and 1976 with legislators, education officials, and interest group representatives) and a 1976 New Jersey poll, Lehne rejects the null hypothesis that the judiciary merely referees legal disputes. Instead, he concludes that an activist court can raise issues and set agenda that must be dealt with by the political branches. It is the treatment of this agenda-setting function of "managed litigation" that offers a modest contribution to a theory of policy formation.

Following the California Supreme Court's decision in *Serrano* v. *Priest* (5 Cal 3d 584, 1971) until the U.S. Supreme Court's ruling in *San Antonio Independent School District* v.

Rodriguez (411 U.S. 1, 1973), a number of state courts overturned their jurisdictions' school finance laws. *Serrano* held that, since there was a significant disparity in district tax bases, the law that relied heavily upon district revenues to fund public education denied children and parents equal protection of the laws. The *Rodriguez* case, however, upheld the Texas district funding law as a rational accommodation of various interests for raising school revenue. This definitive ruling, handed down after arguments had been completed in the *Robinson* case and while the New Jersey Supreme Court was considering its decision, prevented the court from relying upon the *Serrano* rule. Paul Tractenberg, Professor at Rutgers Law School and counsel for the NAACP and the ACLU (amici in the *Robinson* case) reports in *Law and Contemporary Problems* 38:312–32 that the plaintiffs-respondents had based much of their argument upon the *Serrano* principle: there was a large interdistrict disparity of wealth in New Jersey and considerably more than the national average of school funds (67 percent) came from district revenue. Fortunately for the plaintiffs, however, they had also argued enough on other grounds to permit the court to base its decision on a provision of the state constitution guaranteeing a "thorough and efficient" education to all New Jersey residents.

After six earlier decisions failed to encourage adequate funding for the school system, the New Jersey Supreme Court finally adopted the advice of its former chief justice, Joseph Weintraub, who said: "Any interim remedy imposed by the court in a situation where the legislature refuses to act should . . . be roundly disliked by everyone (p. 129)." On May 13, 1976 the court ruled: "On and after July 1, 1976, every public officer, state, county or municipal, is hereby enjoined from expending any funds for the support of any free public school (p. 155)." Closing the schools during the celebration of the nation's Bicentennial was not what most New Jersey officials desired. But it was this extreme measure that finally persuaded the state legislature, especially the lower house, to fund a school finance program, including an income tax, similar to what had been proposed several months earlier.

Lehne's interest in judicial policy formation extends to a concern over the effects of court activism. Does entrance into the political arena taint the judiciary and reduce its immediate or future effectiveness? In the *American Journal of Political Science* 22:896–904, Lehne and John Reynolds pursue this question with survey data and report that

> judicial activism did not directly and substantially affect public attitudes toward the court, but it leaves unexplored the question of whether court activism affects political opinion in more indirect ways (p. 903).

As far as the *Robinson* cases are concerned, Lehne concludes that long-standing favorable public opinion toward the New Jersey Supreme Court and its recognized professionalism, the low public esteem toward the legislature and its internal divisions, and the alliance of the Supreme Court and the executive prevented an assault upon the court by the public or other governmental institutions.

Almost as an aside, Lehne devotes a chapter to assessing the impacts of *Robinson*. Financially, he claims "almost all the money appropriated to support the school finance program would have been appropriated even if the litigation had never taken place" (p. 173). Educationally, the litigation attracted attention for the establishment of a "thorough and efficient" process, minimum standards, and accountability that might not have been "so well defined, carefully considered, or partially enacted" (p. 179) without it. Politically, *Robinson* increased the activity of education groups and accelerated the reform of the legislature. In summary, *Robinson* did set the agenda by making the issue "persistent, ambiguous, immediate, visible, legitimate, and intrusive" (p. 207).

JEDON A. EMENHISER

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Unsafe at Any Margin: Interpreting Congressional Elections. By Thomas E. Mann. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 116. $3.25, paper.)

"All politics is local." So goes the favorite aphorism of House Speaker Thomas ("Tip") O'Neill. "Congressional elections are local, not national events." So goes the corroborating conclusion (p. 1) of this excellent, pathbreaking study by Thomas Mann. "Congressmen," writes Mann, "are responsible for their own margins of victory or defeat; and the constraints they face are defined largely in their individual districts" (p. 3). And, he adds, this is increasingly so.

Mann is led to this kind of "candidate and local effects" conclusion by the questions he asks and the data he uses. His data come from

98 pre-election telephone surveys taken for the Democratic Study Group in 42 congressional districts (300–400 people per district) in 1974 and 1976. The surveys involve more people in more districts than we have ever had available for analysis. The question which interested the DSG was, in each case, how is our preferred candidate doing and what can he or she do to win? Or, as Mann puts it (p. 12), "What are the determinants of change in congressional district returns?"

Thus, the perspective from which he writes is *the candidate's perspective;* the focus of the analysis is *the district;* the unit of analysis is *the contest*–incumbent and challenger or, in open seats, two aspirants; the object of analysis is *voter choice.* This combination of ingredients imparts a new, candidate-centered, district-level twist to congressional elections research. And the book provides a persuasive argument for further research in its mode.

The subject of the book is voting behavior in congressional elections. Mann argues that our research to date underestimates the importance of candidate evaluation as a source of voter choice–and at a time when candidate evaluation (e.g., the advantage of incumbency) is becoming more important. Mann finds that large numbers of voters both recognize (rather than recall) and evaluate the candidates, positively and negatively, primarily with regard to personal characteristics and job ratings. He uses case studies to buttress his inference that "the political dialogue" within each local context does much to shape candidate image and candidate reputation. What candidates say matters. Campaigns matter, too. And finally, Mann argues–both by explaining party defection and by modeling candidate preference–that, at least, "public evaluations of the candidates are very important in congressional . . . elections" (p. 70) and that, at most, "votes for Congress are determined primarily by the reputation of the candidate" (p. 71).

Nothing in the book runs contrary to the widely researched view that incumbents have an advantage over challengers. Nearly every chart, together with the author's discussions of incumbency, confirms that view. But, more strikingly, Mann's evidence warns us on almost every page that "incumbency is a resource to be exploited more or less effectively, not an automatic advantage" (p. 4). Most of his results are displayed by providing both a mean and a *range,* which gives the highest and lowest value for all districts. Although the distributions between these end points are not revealed, the wide ranges point to a vast gap between effective and ineffective incumbents–a gap which exists among challengers and among candidates for open seats as well.

Again, this emphasis on the variability with which incumbents exploit their incumbency stems from the purpose of the research. From the candidate's view, what matters is what happens in one's own district. In a chapter on election outcomes, Mann notes that candidates worry about the interelection swing in their own districts, more than about that national swing which has been the subject of our aggregate election studies. He demonstrates that interelection swings are anything but uniform across districts; and he argues that the candidates and other local factors must be studied if we wish to explain these variations. He repeats the argument after demonstrating the varying degrees of success candidates have in exploiting the basic distribution of partisans within their districts.

This is not a definitive book. Data limitations keep it from becoming such. For the methodologically critical, the concentration on marginal districts, the exclusive use of pre-election surveys, the small number of usable districts in some cases, the small number of cases in some cells will be cause for withholding final judgment. And rightfully so. But even with its imperfections, this unique data set has important virtues. It reminds us that useful electoral data can be unearthed in places other than in our great national collections, and that such data can be mined for the purposes of discovery, if not of confirmation. It reminds us, too, that such data, analyzed with care, can help chart future research directions for our great national enterprises–in this case toward comparative, district-level analyses of voter choice. People at the University of Michigan should read this stimulating book. So should everyone else interested in the subject of congressional elections.

RICHARD F. FENNO, JR.

University of Rochester

Marcus Foster and the Oakland Public Schools: Leadership in an Urban Bureaucracy. By Jesse J. McCorry. (Berkeley: University of California Press, 1978. Pp. xv + 163. $10.95.)

Marcus Foster, the first black superintendent of a major urban school system, was assassinated in November 1973, just three years after arriving in Oakland. Because Jesse McCorry, a personal acquaintance of Foster, was given close access to Foster's administrative staff, and because he treated that confidence with the seriousness and integrity it warranted, the superintendent's record has been given a permanence that his zeal and sense of purpose deserve. At the same time McCorry provides us with an unusual study which is once intimate in its appreciation of the "internal politics" of a big-city school administration and detached in its scholarly analysis.

The politics of the Foster recruitment demonstrates the bizarre set of circumstances that seemed necessary before Oakland (or any other big city?) could become the first to have a black school superintendent. Before awarding the position to Foster, the school board, responsible for schools with a 75 percent nonwhite enrollment, had offered it to three different white prospects. In the process it had hopelessly bungled the recruitment effort, exposing itself to widespread public ridicule. Only then did it search assiduously for a black person.

Once in office, Foster suffered the dual disadvantages of being both a black and an "outsider," the latter handicap perhaps being the more severe. He had three major objectives: increased community involvement, especially in the processes of principal selection; administrative decentralization; and the implementation of the then-fashionable planning, programming and budgeting system (PPBS).

Although McCorry gives the policy only passing attention, Foster had the greatest success in increasing ostensible community involvement. But this success was possible, curiously enough, only because community participation "did not affect the normal routines of the system's staff" (p. 145) and because funds to support the citizens' committee were provided by outside foundations. Decentralization of administrative practices to regional offices encountered more severe obstacles. Old-line staff members, with connections to board members, objected to the loss of power and status entailed in decentralization. Not only was Foster blocked from choosing one member of his team of regional administrators, but also regional offices were never given much staff support or assigned their anticipated set of responsibilities. PPBS, if anything, was more of a disaster. Apart from Foster's own limited understanding of budgets and PPBS' inherent limitations, there was, once again, the opposition of a budget office deeply convinced that standard routines were preferable.

After three years in office Foster could hardly conclude that he had made the same impact in Oakland as superintendent that he had once made in Philadelphia as principal. Only six months before the end Foster himself publicly complained:

> Blacks become superintendents only in cities like mine where the percentage of non-whites has reached 78 percent. Black educators don't get called into "cushy" jobs.... Then when you can't make it, they say, "I told you those niggers can't do it!" (p. 149)

McCorry's work is sober reading for any reform-minded school administrator who believes that urban bureaucracies simply require new leadership. Yet McCorry's analysis has its own limitations. It is not simply that quotations from well-known students of organizational theory and public administration too often substitute for the author's own analysis and interpretation. Nor is it primarily McCorry's inability to choose between characterizing Foster as a dynamic leader or as a frustrated superintendent. Biographers of John Kennedy are often left in similar confusion. What is most troublesome is the way in which the book's analytical standpoint wavers. At the close of the preface, McCorry makes the provocative, if improbable, claim that "bureaucratic resistance may not be the villain which so many advocates of change claim it to be" (p. xiii). But instead of developing this line of argument he provides a fine, first-hand account of administrative infighting. Certainly, "the problem" is always "a lack of knowledge" (p. xiii), but that is too general a formulation to help account for Foster's particular problems in Oakland. Uncertainty is the necessary but ubiquitous condition under which bureaucratic politics thrives.

PAUL E. PETERSON

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The Rise of the Sunbelt Cities. Edited by David C. Perry and Alfred J. Watkins. (Beverly Hills, Calif.: Sage, 1977. Pp. 309. $18.50, cloth; $7.95, paper.)

This is a provocative book. Its essays go substantially beyond popular treatments of southward shifts in population and economics. They put the rise of sunbelt cities into the context of American urban history, and clarify the events taking place in various urban strata. The book says as much about the old northeast as about the new south. It provides useful data analysis, fine case material, interesting predictions, and challenging dispute between opposing prescriptions for urban policy.

Two themes crop up throughout the book: (1) Like American cities generally, those of the sunbelt thrive according to their success in material production and distribution. To do well, American cities must be good for business. There is little room in the United States for cities whose principal functions are as capitals of administration, religion, or culture. Sunbelt cities have been better for business than northeastern cities in recent decades; they have provided more freedom to entrepreneurs, and they have burdened their entrepreneurs with fewer demands on behalf of the unproductive poor.

(2) The economic progress of sunbelt cities has not been felt equally throughout their social strata. There remains substantial poverty from the past. Along with aggregate economic growth, there has been some widening of the spread between upper- and lower-income groups.

The final three chapters explicate some prescriptive implications of these themes. Gurney Brechenfeld shakes up a reader who might have been lulled by the left-of-center sameness in most earlier pieces. He warns the cities of the sunbelt to avoid the pitfalls of the old North. The resourceful leader should "cut expenses, curb crime, perk the place up with new action, and win battles with militant municipal unions.... The more aid cities provide for the poor, the more of them they invite to congregate within their borders.... One result is the triumph of misguided altruism over economic common sense." Murray Bookchin and then the editors urge cities to recognize other values than those of the marketplace. They evade the sharp thrust of Breckenfeld's beggar-thy-neighbor prescription, however, and thereby help to legitimize it. When Perry and Watkins write that the social unrest of a declining northeast cannot be hidden by the rise of the sunbelt, they are only partly correct. The pro-business, anti-welfare policies of the sunbelt do seem to work at the local level, supported by federal structures of government and wide respect for local autonomy that allow the cities of one region to gain at the expense of others.

One of the bright spots in the collection is a case study of Houston entrepreneurialism by William D. Angell, Jr. It is an exceptional piece that fits implicitly into the themes developed by other authors. Another gem is Robert Cohen's analysis of sunbelt dependence on national and multinational corporation giants financed and directed from outside the South. He adds substance to the impression that sunbelt growth has been less an indigenous movement arranged by resourceful managers, and more a case of outsiders taking advantage of a ripe opportunity.

While there is much in this anthology to provoke discussion, it lacks systematic treatment of its own key themes. It suffers from the generic problems of uneven quality and anemic editing. The editors do supply cogent introductions to each section of the book, but they should also have pruned the contributions of several authors. At times the editors' comments are more satisfying than the articles they mean to introduce. There is considerable repetition of general themes from one chapter to the next, and too little progression in the analysis. Several chapters have cogent data analysis or interesting case materials, then lose their sharpness with a flaccid rehashing of themes that are treated elsewhere. The editors deserve thanks for departing from the northeastern bias that they perceive in the urban literature, but they might have done better with a wider base for their own efforts. Six out of 13 contributors come from the University of Texas. A more aggressive search for talent might have exposed the anthology to interesting variants like Miami, Jacksonville, Phoenix, and Los Angeles. All told, the book will carry its weight as a supplement to urban politics courses at the undergraduate level and several of its pieces may find themselves widely cited by specialists in the field.

IRA SHARKANSKY

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What is Governing? Purpose and Policy in Washington. By Richard Rose. (Englewood Cliffs, N.J.: Prentice-Hall, 1978. Pp. x + 173. $5.95, paper.)

This modest-sized paperback is more effective in its parts than as a whole. Richard Rose's attempt to organize the study around the concept of governmental purposes and the elaboration of 18 "models of governing" in terms of purposes does not come off too successfully. For one thing, the notion of "purposes" is too thinly and imprecisely defined and, in application, it apparently embraces means as well as ends. For another, the study's use of the terminology of "models" has no persuasive rationale and no clear utility; students are more likely to be confused than helped by it. Fortunately, the book's strengths—and they are considerable—emerge intact in spite of these limitations.

Considered apart from the problems related to its central organizing theme, Rose's study offers a brief but cogent review of a large variety of approaches and emphases in the study of American politics and policy. The 18 "models of governing" (some of which are sketched below) provide, in the aggregate, extensive coverage and touch on many subjects emphasized within introductory courses on American government. The volume should prove useful for providing context and perspective to beginning students of American politics.

The study's coverage includes examination of opposing views on whether government should seek to conserve or change society; whether government should aim to produce more wealth or more welfare for its citizens or more legitimacy or power for its public officials; whether policies should reflect individual preferences, group demands, an abstract public interest, or the interest of public officials or agencies. In addition, Rose treats varieties of decision making (satisficing, incrementalism, etc.) in each of the national political institutions as well as administrative implementation, planning, and policy evaluation.

On each of these subjects, Rose connects the "model" to the stage of the governing process, to the political institution, and to the category of public policies to which it is most relevant, and notes whether the "model" is or is not distinctive to American politics and what interests benefit the most from its application. Rose's necessarily abbreviated discussion of these matters should stimulate abler students to seek fuller and more systematic comparison.

Readers of this work should come away with a solid awareness of the complexity of American politics and policy and, consequently, of the complexity of studying those phenomena. At the same time, the study strengthens the students' ability to cope with that complexity by conveying, with brevity and good sense, diverse ways of undertaking such a study.

ALLAN P. SINDLER

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Goodbye to Good-Time Charlie: The American Governor Transformed, 1950–1975. By Larry Sabato. (Lexington, Mass.: D.C. Heath, 1978. Pp. xvi + 283. $20.00.)

Sabato has undertaken the task of testing the "Goodbye to Good-Time Charlie" thesis: that governors of American states have increased their powers to govern; have exerted more influence both within their states and in national affairs; have moved from being a roadblock within the federal system to being key negotiators and coordinators for all levels; and, most significantly, have substantially higher qualifications in terms of vigor, training and performance than their predecessors. For active governor-watchers this is not news, but Sabato's examination of the thesis and its supporting data provides a solid basis for upholding the argument.

Sabato approaches the question by gleaning information obtained from other researchers and extensive archival probing, by conducting semi-structured interviews with governors, former governors and many who have worked with or watched governors over the past decade or so, and by a careful reading of the popular press. The result of these approaches provides a rather realistic analysis of just what has been happening in the states and the governors' chairs over the 25-year period.

Sabato has developed several useful lines of analysis in pursuing the thesis, some borrowed and some new. Taking a leaf from Joseph Schlesinger's *Ambition and Politics* (Chicago: Rand-McNally, 1966), he gives a very persuasive portrait of the change thesis by examining the governors' backgrounds and career patterns into and out of the governor's office. The closing chapter, "The Governorship as Pathway to the Presidency," caps this by comparing the governorship to the vice-presidency and the U.S. Senate as stepping stones to the White House.

Sabato's rating of the top governors during the period under study (37.5 percent or 117 of the 312 who served), while bringing forth some surprises, does fit with some of the "gut feelings" that observers have had about performances in the governorship. Important here are the characteristics associated with these "top" governors—they were younger, better educated, and had legislative experience to a greater degree than their lesser-ranked compatriots. Further, by definition they were not stand-patters, but governors "of conspicuous ability and competence whose term was characterized by personal hard work and firm dedication and who diligently attempted (even if unsuccessful in part) to meet the needs of the people of his or her state" (p. 51).

Comparisons of this top governors' ranking and Sabato's extensive analysis of the key issues leading to the defeat of governors at the hands of the electorate during the period are most significant. They clearly show that being an outstanding governor by attempting to meet the needs of the people can lead to becoming a former governor—as meeting these needs too often translates into new taxes or tax increases and voter rejection. While the concept of "tax-loss governors" has been asserted, analyzed and questioned, Sabato's state-by-state review seems to support its validity—and close observers know governors believe it to be true and make decisions with that as a premise.

Sabato makes careful and systematic analyses of the impact of some of the goals of reformers on the governorship: off-year elections and increasing gubernatorial tenure. He also explores the effect of ticket splitting, increasing campaign costs and party competition on the changes in governors and their roles. An excellent bibliographic essay and bibliography are contained in the appendix.

This study pertaining to a major office in our federal system is very well done and uses the most relevant data available to support its thesis. While the amount of factual data presented is sometimes overwhelming, I suspect the presentation has considerable utility to a wide audience including both practitioners and academics.

One fault of this presentation may be the basic assumption underlying Sabato's argument. There is no question that the states have come a long way over the past decade to get their own houses in order, to move away from former Governor Terry Sanford's overdrawn characterization of them as indecisive, antiquated, timid and ineffective, not willing to face problems, and not interested in their cities (*Storm Over the States*, New York: McGraw-Hill, 1967, p. 1). Constitutions have been revised, structures reorganized, legislatures strengthened, and gubernatorial power increased. State governments have also dropped much of their reticence toward becoming actively involved in policies and programs at the local and national level, and have increased their taxing powers significantly to meet the cost of a more active stance. Sabato carefully documents all these trends.

However, these are but trends. Looking across the states and their governors, one can see that considerable variations in their status still exist; despite the new breed of governors and better weapons at their disposal, much remains to be done. Actors and agencies at other levels must become aware of the governors' capabilities and remove age-old blinders which restrict views of governors' roles. Concomitantly, increasing evidence indicates that governors may now be even further restricted in what they can do by various federal mandates, guidelines, standards and directives.

Sabato has incisively argued that states are growing stronger and their governors are of increasingly higher caliber and ability. It may be a case of having to run faster just to keep up with the neighbors, but this may be good news also.

THAD L. BEYLE

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The Giant Jigsaw Puzzle: Democratic Committee Assignments in the Modern House. By Kenneth A. Shepsle. (Chicago: University of Chicago Press, 1978. Pp. viii + 333. $30.00.)

The subject of committee assignments has attracted at least its fair share of attention. Interviews have been conducted with Committee on Committees members and with freshman members of Congress after their first assignment. Turnover has been analyzed along with patterns of transfer to and from committees. The outcomes of the process have been studied at length—for freshmen and nonfreshmen, marginal and safe members, southerners and nonsoutherners, liberals and conservatives, party supporters and mavericks. New data were added with the confidential "request lists" of members seeking assignments: the requests could then be compared against the assignments received. A 1973 *Review* article by Kenneth Shepsle and a co-author analyzed these requests for House Democrats in four congresses. *The Giant Jigsaw Puzzle* draws from and is an expansion of that earlier work.

The book pursues three separate endeavors. It offers, first, a wide-ranging review of the assignment process and selected congressional practices, including a chapter on congressional history, a summary of recent reforms, and considerable descriptive background material throughout. It offers, second, an empirical analysis using the request lists (expanded for House Democrats from 1958 through 1974) and supplementary interviews by the author and others. It offers, third, a mathematical specification and testing of some arguments about the assignment process. These arguments are called theories. Thus chapter 4 in a "theory of freshman requests" argues that freshmen will request committees which reflect their constituency interests or their personal policy interests. Chapter 5 discusses opportunity costs, "wealth effects" (the present committee assignment) and "the status of the queue" (the assignment sought) and finds that nonfreshmen already holding good assignments will transfer only to the most prestigious committees whereas nonfreshmen not holding good assignments are more likely to seek transfers to other committees. The "theory of requests," "theory of the negotiated committee structure," and "theory of assignments" (see p. 7) combine to tell us that members will seek assignments reflecting their own interests and that party leaders and the Committee on Committees will try to accommodate these requests wherever possible.

The descriptive chapters (essentially 2, 6, 7, 8, and epilogue) supply some highly interesting detail on the workings of the process, though much of this will be familiar to serious students of Congress. The mathematical sections (chapter 4, portions of 6 and 9 and the appendix to 7) are meticulously constructed, useful illustrations of how complex political questions can be translated into testable form. The empirical work (chapters 3, 5, and 9), partially previewed in the 1973 article, has been deepened and extended. Of particular interest is the finding that impersonal factors relating to the assignment (such as the amount of competition for the post and for the other posts requested, whether the regional zone of the requester already has "sufficient" representation on the committee, and whether the requester's predecessor in the district held the same assignment) are important to the success of the request, whereas factors relating to the requester (party support, region, electoral safeness) are not found to be important. (See chapter 9.) This finding supports the thesis being developed throughout the book that the jigsaw is too large and formidable a problem to permit deviations into the personal or ideological from impersonal and institutional-level criteria. It also supports the established wisdom about Congress as an institution placing conflict management and accommodation above any particular policy concerns or political disputes.

The jigsaw thesis should be regarded as tentative. The measures selected—especially for the requester's characteristics—may not constitute an adequate testing of the propositions. For example, a general measure of party support for nonfreshman requesters is the only measure of ideological or policy influence tested and is found unimportant. Shepsle argues that "Committee on Committee members are, in the main, partisan advocates, so it was anticipated that, all other things being equal, they would be most disposed to those requesters who were strong party supporters" (p. 217).

The Committee on Committees, however, is itself selected to represent the very diverse elements of the congressional party and so might as easily be expected to converge around the average as around the strongest party supporters. Strong party supporters in the Democratic Party are the liberals; but the Ways and Means Democrats, who comprised the Committee on Committees for the time span of the study, were rarely accused of a particularly noticeable liberal bias. More seriously, however, committee-specific ideological or policy positions were not investigated and these might be expected to have more influence than general party support scores on who gets what assignment.

The variety among committees also requires attention. Committees vary in subject matter, member goals, and political environment. Even the large number of middle-prestige committees ("semi-exclusive" committees) vary from one another and might well attract different requesters and different treatment in assignments. So while the semi-exclusive committees are broadly distinguished at points in the study from the non-exclusive committees (concerning which few requests are made) and from the exclusive committees (concerning which few requests are granted), the potentially more interesting variation within the middle-range category is not examined. If, for example, as past writers suggest, there are "pork-reelection" and "policy" committees in the large middle category, then many of the empirical findings and conclusions of the study may need to be qualified by this distinction.

We can hope that ways will be found to preserve anonymity and make the request data available to a wider research community. Necessary follow-up studies, with attention to measurement problems and committee variation, could then be conducted.

The book is not a major theoretical advance nor a major empirical one, though it offers an important hypothesis on the assignment process along with valuable data and rich detail. It is, however, a work of considerable effort and talent by an author skilled in mathematical and statistical design and at ease with diverse forms of congressional research. It is a very thorough, at times innovative, and always interesting account of the committee assignment process. While the price of the book will limit its use, it can be read with profit by students of Congress and of mathematical modeling applications.

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Comparative Politics

In a Moment of Enthusiasm: Political Power and the Second Stratum in Egypt. By Leonard Binder. (Chicago: University of Chicago Press, 1978. Pp. xxii + 437. $22.50.)

This painstaking study of Egypt's "second stratum," the rural middle class, is a remarkable contribution to the literature on political development. While not breaking new theoretical ground, it transcends existing liberal and Marxist paradigms to offer an empirical explanation for the apparent stability of an authoritarian regime that neither school could dimly suggest. In the author's words,

> If . . . we find particular phenomena, particular historical events, and particular statistics commonly referred to by the analysts of both persuasions, it may just be possible that some piece of social knowledge may be partly freed of its relativistic and polemical character by the deliberate method of collating middle range theory which deals with the phenomena in question regardless of the major paradigm from which it may be taken (pp. 10–11).

Binder is no blind empiricist–indeed, his work will be a classic in the development field precisely because the meticulous data manipulation usually has a theoretical rationale behind it–but the thrust of his argument is atheoretical. To put it crudely, the rural middle class stabilized Nasser's regime and Sadat's (at least up until time of publication, fortunately for the author) and enabled them to mitigate urban class conflict without granting significant participation to either the urban middle classes or the proletariat. To understand and interpret Egypt's model of political development, we must therefore analyze the social, occupational, historical, geographical, and political characteristics of this "second stratum" which Binder, following Gaetano Mosca via James Meisel, defines as "the instrument without which the rulers cannot rule" (pp. 12, 16, 406).

The analysis entailed fascinating detective work. From the Golden Register of the National Union, Binder studied the names of the 27,936 members of the 4,191 village and town committees elected in 1959 in Egypt's 16 agricultural provinces. He hypothesized that his second stratum would include most families two or more of whose members appeared on a given committee. His three coders could agree upon 5,498 individuals constituting 2,223 family sets, almost one-fifth of the total sample. Unfortunately committee size varied–from 3 to as many as 35–obviously skewing the distribution of the putative second stratum (cf. Table 16, p. 95) in favor of the larger committees, where family sets may have been clans rather than families of notables. Binder was aware of the difficulty (p. 75) and tried to validate the family sets as a rough and ready indicator of second-stratum status. The family set member was more than twice as likely as the nonmember to have been elected to district office in 1959 and to have served as a member of the parliament elected in 1957 (p. 77). Moreover, these relationships held up independently of the size of the village committees. In general, too, family set members displayed higher occupational status than singles.

Binder then searched outside his sample of names, offices, and occupations for more concrete evidence of his second stratum. The obituary notices of *Al-Ahram* over a six-month period (1960–61) were mined for family networks of ancestors and survivors of the deceased. In 99 of 157 cases at least one person named in the obituary was found in the Golden Register, and as many as 60 were members of family sets. In 42 of these cases, at least two family members could be located in the register. "Hence, in 70% of the relevant cases it was possible to confirm that the family set is indeed a family set" (p. 154). But who were they? The historical dimension was the most exciting aspect of the quest.

The second stratum was supposed to be those 700,000–1,000,000 Egyptians in nuclear families owning 20 to 50 feddans–some 3 percent of the total population (pp. 156–57). Binder got one of his students, Sidney Chesnin, to analyze the names of izbah owners–proprietors of certain sorts of rural estates of at least 50 feddans–mapped in 1930. Chesnin found that 22.9 percent of the members of family sets had been izbah owners in 1930, compared to only 3 percent of the singles (pp. 106–08). Binder himself probed further back in time, locating and analyzing the names of rural notables appearing in Ali Mubarak's *Khitat,* a survey compiled in the mid-1870s of landed grandees and rural notables, among other things, by one of Egypt's most prominent civil engineers. Of the quarter of the family names definitely appearing in the Golden Register, some 32 to 34 could be traced to 40 family sets, while 25 others could be traced to 26 individuals (inferred from somewhat confusing data, pp. 116–18). Thus members of family sets were more likely than the singles to display a century of historical continuity.

Even more fascinating was the historical search for members of past parliaments whose descendants appeared in the Golden Register. With the help of another graduate student, John Anderson, Binder tracked down the names and occupations of the MPs serving from 1866 to 1882 and 1924 to 1950. Of the 2400 seats in the agricultural provinces, 37.5 percent were held by ancestors of members of the National Union committees, and 75 percent of the descendants were among the fifth of these members who belonged to family sets (pp. 130, 140). Some of these rural families, like those observed by Ali Mubarek, clearly retained their elite status over the generations, for the families of up to 60 percent of the nineteenth-century MPs persisted in the 1959 sample. Yet the historical search did more than merely confirm the importance of family set members in 1959; it also may have unmasked their principal political function. The percentage of "ancestor" MPs jumped from 17.4 to 58.7 in 1931, the year Ismail Sidqi elected a docile parliament to buttress his authoritarian regime (pp. 131, 136). Marshaling a variety of data concerning the landholdings and political affiliations of the MPs, Binder concludes quite plausibly that his second stratum "developed from the seed planted in 1931, that it was formed in the image of the electoral struggle of the 1930's and 1940's, and that it was decisively defined by the policy of excluding important former Wafdist politicians in the aftermath of the Naguib-Nasser dispute of 1954" (p. 142). Indeed the 1959 data throw retrospective light on the pre-Nasser period, especially upon the competition after 1931 between the Wafd and the palace to recruit the rural notables.

Ultimately, however, it is less clear what this study of the rural middle class tells us about contemporary Egypt. Part 2, "The Geographical Distribution of the Second Stratum," gives us much information in the form of bivariate distributions, correlations, and maps about the National Union's provincial elite—probably more information than the average reader would care to digest despite the suggestion on p. 281 that these may be only the beginnings of "an important subject for future research." The relationships between second stratum strength and various indices of modernization could have been presented more parsimoniously using multivariate regression analysis (with provinces or provincial sets as dummy variables), and factor analysis might have simplified structure of independent variables while minimizing collinearity. But the major difficulty I have with this book is not so much the methodology—though note, for instance, the strange inferences about occupational mobility derived from the inherently skewed data of obituary notices (pp. 160–67)—as the substantive interpretation of the second stratum's political role.

The second stratum is variously defined as (1) the instrument without which rulers cannot rule; (2) the class that "can arouse, in itself and in the masses, a moment of enthusiasm in which it associates and mingles with society at large, identifies itself with it and is felt and recognized as the *general representative* of this society" (p. 19, citing Marx's "Critique of Hegel's Philosophy of Law"; (3) "the pool of qualified persons from among whom the important and not so important officials are chosen" (p. 28); and (4) "a process [of social change] rather than a structure" (p. 279). I suppose a rural middle class, as defined by landholdings, could turn out to be all these things, but the data, which represent village, district, and provincial elite families, can only tell us about its roles in the countryside and its adaptation to urban influences in the northern provinces, which tend to be more exposed than those of Upper Egypt. No evidence is presented in support of the hypothetical roles of the second stratum suggested by the first, second, and third definitions. The third definition is manifestly misleading: not all Egyptian important and not-so-important officials come from rural middle class families, as defined by 20- to 50-feddan landholdings. Perhaps this social category is over-represented in the national political elite (however it may be defined), but so what? So also is the urban "bourgeoisie and aristocracy" defined by Samir Amin. Perhaps at the very top, say among Nasser's 50 most influential political hacks, most of whom would have been army officers, the rural middle class would be preponderant, but we are not given these data.

The Marxist "moment of enthusiasm" is far more problematical. Perhaps the Nasserist regime did attempt "to transfer the spirit of village Egypt to the seat of power in Cairo"—though Sadat appears to stage-manage the effort more self-consciously than Nasser ever did—but Binder also agrees that neither president could really mobilize the masses, much less emancipate society in the spirit of the early Marx. Indeed, neither the National Union nor its successor, the Arab Socialist, spearheaded social change so much as prevented it by occupying any available political space. The second stratum may indeed be conceived as a mediating instrument between state and civil society, but then Binder should have turned for his theoretical inspiration not to Marx but to Hegel, who posits a leisured and public-spirited landowning stratum (not necessarily the Junkers!) as the spiritual glue binding a differentiated civil society with the state apparatus outlined in his *Philosophy of Right.* Binder's references to Marx seem forced, as if it is necessary to pamper other interpreters of Egyptian society who are Marxists. The comparisons between Nasser and the Louis Napoleon of Marx's Eighteenth Brumaire are particularly ambivalent. On pp. 22, 399, and 401 Nasserism gets compared to Bonapartism, though the parallel suggested on p. 22 would seem to be with the first Napoleon, not his imitator; on pp. 304 and 398, however, Binder corrects himself, for the regime "turned against the bourgeoisie." He quite rightly argues that the peasants were not Marxian sacks of potatoes, but the mystery remains: in what sense might Nasser have reflected (or conjured up) a rural middle class "moment of enthusiasm?" Perhaps what Binder really meant was that "Nasser's rural support was . . . more enthusiastic than materially interested"—a useful source of support because it was relatively undemanding (p. 304). Despite appearances, the rural middle class ("lucky Pierre" on p. 7) is not the principal beneficiary of the regime, but some of its urban kinsmen are supposedly favored over other, older professional, technical, and functionary groups (p. 377). Maybe so, and maybe the favors make the rural folk proud and happy back on their izbahs. But this book does not offer one shred of evidence in support of this interesting hypothesis.

In the last analysis, perhaps these folk really were the "instrument without which the rulers cannot rule," and Binder presents an ingenious theory of their relevance to Egyptian political development. They enabled Nasser and Sadat to minimize the political influence of any nascent liberal bourgeoisie or proletariat and to dampen any conflict between these urban forces. That is why liberals and Marxists find it so difficult to interpret Nasserism. "Egypt is the prototype of the situation in which there was a significant hiatus between the urban and rural political arenas" (p. 404), and this situation, one might add, enabled successive regimes to divide, rule, and paralyze the nascent civil society. But if the "social linkages between urban and rural segments become more important than the relatively weak connections between the two political processes" (p. 404), an analysis of the urban process was needed to confirm them. As it is, Binder gives us only half of the story. The counterpoint to his interpretation of Egyptian politics is the philosopher's insight that the owl of Minerva takes flight only as dusk is falling, that is, that the second stratum is already losing its historical role. The understanding, backed by meticulous research over a 15-year period, was no mean achievement for an American political scientist. This book will be a minor classic and probably also a sort of twin testament to the epochs of Egyptian rusticity and American behavioralism.

CLEMENT HENRY MOORE

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Power and Manoeuvrability: The International Implications of an Independent Scotland. Edited by Tony Carty and Alexander McCall Smith. (Edinburgh: Q Press, 1978. Pp. xv + 185. £6.95.)

The Scottish independence movement has generated a growing body of literature. This collection of six articles considers the extent to which an independent Scotland would be free to alter international political and economic arrangements to suit its interests. Detailed scenarios illustrate the constraints and likely options in politics, land control, oil, trade, and monetary policies. All are serious and scholarly analyses, though nationalist sympathy is not lacking. Americans are generally ignorant regarding this situation, and, whether one thinks that Scotland should or will be independent, understanding what might occur if it were, is of considerable practical and theoretical importance.

In his chapter, "Politics," Stephen Maxwell outlines strategic and tactical moves Scotland might make and analyzes the likely reactions of Westminster, the EEC, the U.S., the oil companies, OPEC, and the Nordic nations. He illustrates with experiences of other small states—Norway, Eire, Switzerland, Holland, Iceland, and Canada. The scope and detail of this chapter are impressive, the writing crackles with good argument and the coverage provides an excellent short course in the topic of Scottish Nationalism. Oil was the catalyst that set things in motion, but Maxwell argues that there is a connection between the oil situation and a long-standing Scottish grievance, the rape of Scotland's resources for the good of Westminster. Rapid extraction of oil to set right the U.K. economy is the most blatant example; fishing is another. "Scottish waters contribute 50% of the total UK fish stocks and nearly 30% of the EEC stocks" (p. 5). That Westminster should have bartered away the access rights of Scottish fishermen in return for entry into the EEC rankles. For Maxwell, these uses of Scottish resources to bail out a failing U.K. explain the apparent paradox in which there is a splintering of the U.K. along traditional ethnic lines at the very moment when modern economic integration of Europe is taking hold. To wit, as long as Scottish resources are being bartered, Scots should control the bartering. In an increasingly interdependent Europe, such leverage would be worth having.

Alexander McCall Smith directs attention to two points of international law that are relevant and problematic: the right to self-determination and state succession. The former may be a problem for Scotland if it can be argued, and some insist it can, that "it is in no sense prevented from full, equal and real participation in the multinational state. . ." (p. 45). The problem of state succession takes us directly to the crux: ownership of North Sea oil. As Carty puts it:

> "Traditional state succession involved only the substitution of one sovereign, who was often a monarch, for another, and left the legal relationship between individuals intact." . . . [This] is now highly controversial precisely because the distinction between private rights and public institutions is widely regarded as unsound (p. 115).

The first goal of an independent Scotland would be to follow Norway's example and impose strict control over the extraction rate of oil. Carty argues this would be impossible without claiming ownership. The licenses granted by the U.K. are ambiguous on this, but appear to extend to the oil companies control over depletion rates. A possible model to follow is found in the "OPEC revolution." There, the old concessions explicitly granting proprietary rights were scrapped for contracts in which the companies extract, refine and distribute the oil at a rate and price determined by the states who retain control. To follow this path, Scotland will risk the wrath of the oil majors and their influential backers, the large American banks. Should the companies refuse to deal, or should they choose to make a stand by scuttling their platforms, Scotland would be hard pressed to develop an extraction capacity. Even short of that, as Carty rightly notes, renegotiation is made more difficult since Scotland can hardly claim, as Ghana and the OPEC states could, that the concessions were unfairly wrested, through shrewd dealing, from a primitive and unsuspecting people.

Oil, with its guarantee of surplus balance of payments, is the heart of two articles concerning trade and money in the small state. Dermot McAleese and John Purvis assume that Scotland will be a rich state whose problems will be to contain domestic demand and to invest oil proceeds against the future while avoiding inflation. Interesting analyses, intelligible to the non-economist, argue that an aggressive policy of competition in free trade, limited membership in the EEC, and a monetary policy alert to that of England—then as now the main trading partner—would be prudent courses of action.

These are fascinating and informative pieces that have about them the kind of insight and argument that illuminated our *Federalist Papers*. Critics can note that the authors tend to take lightly the tactics available to Westminster. Also, a devolved Scottish Assembly might well over-extend the SNP and dampen enthusiasm among its voters. The Shetland Islanders' apparent willingness to throw in with England, taking with them two-thirds of the oil, deserves more attention. And finally, the process of gaining independence may take so long that the oil could be nearly gone by the time its control fell to Edinburgh. But these are minor objections to a very good book. Together with Donald MacKay's collection of scenarios concerning domestic policy options, *Scotland 1980* (same publisher), *Power and Manoeuvrability* is well worth reading.

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Germany at the Polls: The Bundestag Election of 1976. Edited by Karl H. Cerny. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1978. Pp. 251. $4.75, paper.)

No West German election to date has received more scholarly attention than the election of 1976, even though, as most observers would agree, it was not one of the most interesting or consequential elections since 1949. 1976 lacked the suspense and "new frontier" spirit of 1969 or the frenzy and issue focus of 1972. In the end, the 1976 election maintained the balance of power, albeit with a reduced margin; the CDU/CSU had won, as Kaltefleiter put it, without achieving victory.

Germany at the Polls is mostly concerned with political parties and their role in the electoral game. In the opening chapter, Loewenberg does an admirable job sketching the party development in West Germany since 1945. Conradt focuses on the parties in the 1976 campaign, especially their campaign strategies, and offers an assessment, based on aggregate data, of the outcome. Like Loewenberg, Conradt finds kind words for the "maturity" of the West German party system. A "stable turnover system" has evolved in which party coalitions have alternated in power and in which such alternations remain likely without loss of system stability.

The contributions to the volume by Sontheimer, Kaack, Kaltefleiter, and Noack review in more detail the election campaigns waged in 1976 by the four parties, the SPD, F.D.P., CDU, and CSU. Each of these chapters covers the intraparty ideological struggles, such as between the *Jusos* (Young Socialists) and the party establishment within the SPD, the conflicts over the drafting of the party platforms—with due attention to the controversial "freedom versus socialism" slogan of the CDU/CSU—and the presumable effects of such slogans on the voters' choice at the polls. Yet, except for the Kaltefleiter chapter, none of these contributions exceeds the level of quality journalism; Kaltefleiter draws on electoral surveys to highlight voter evaluations of the key campaign actors and the issues of concern to the mass public.

Two contributions of a very different sort round out the collection. The chapter by Schoenbach and Wildenmann is devoted to the mass media, particularly the coverage of the election campaign by the prestige press in the Federal Republic. The political editorials of these newspapers are examined by systematic content analysis. The authors draw some intriguing comparisons between the "profile" of that coverage and the profile of party platforms, speeches by party leaders as well as party evaluations on the part of the mass public. The final chapter, by Kaase, takes up some of the issues—as well as promises—raised by the spread of public opinion polling in the Federal Republic. Kaase notes the extensive collection of electoral surveys available to the scholarly community—through the service of the *Zentralarchiv* in Cologne, and through the ICPSR in Ann Arbor—but deplores the inadequate infrastructure for conducting university-based electoral research in West Germany.

In a volume like *Germany at the Polls* some overlap between chapters is unavoidable, but the degree of redundancy in this volume is somewhat annoying. Two nearly identical tables appear in the Conradt and the Kaase chapters (see pp. 41, 46, and 221). The four single-party chapters cover a good deal of the same ground. Moreover, while each of them addresses the question of the outcome of the 1976 election, none is capable of answering it because the focus on a single party's campaign simply precludes that possibility. It is a critical flaw of this volume that a chapter "German Voters at the Polls," i.e., an analysis of voter choice in 1976, using survey data, is missing. To be sure, Conradt approaches the subject and Kaase devotes a good portion of his chapter to it, but this topic deserves far more attention than is allocated.

Not only will those curious about electoral behavior in West Germany be disappointed by this volume, but also those interested in new perspectives on German political parties. The various chapters in this volume add little to our conceptual understanding of parties or party systems, and the reader waits in vain for a new or innovative twist in the study of parties and elections, aside from the media chapter by Schoenbach and Wildenmann. These objections notwithstanding, this volume provides the general audience with a useful introduction to contemporary party politics in West Germany.

HELMUT NORPOTH

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Commandos and Politicians: Elite Military Units in Modern Democracies. By Eliot A. Cohen. (Cambridge, Mass.: Harvard Center for International Affairs, 1978. Pp. 134. $8.95, cloth; $3.95, paper.)

Arthur Schlesinger, the historian, was recently quoted as saying: "Some day, I'm going to conquer the art of writing short books." What Schlesinger has not yet mastered Cohen has—and very well.

Using primarily America's Special Forces (1952–1978), Britain's Commandos (1940–1945) and Special Air Service Unit (1941–1978), the French and Israeli paratroopers (1942–1962 and 1953–1977), the pre-State Jewish Palestinian *Palmach* (1940–1949), and Unit 101 of the Israel Army (1953), Cohen does essentially two things. He discusses the military and political costs of elite units and then offers suggestions for managing them better within the democratic context of civilian control of the military.

What makes this book different from other studies of civil-military relations is that Cohen "looks at discrete units rather than the officer corps of an entire army," and more importantly, he devotes a great deal of time to examining how "politicians interject themselves into the soldiers' roles rather than the reverse" (pp. 16–17). Indeed, this is really the main point of the book.

The politicians not only interject but they protect: John F. Kennedy and the Special Forces, Sir Winston Churchill and the Commandos, Moshe Dayan and the *tsamkhanim*, or paratroopers. In different countries and circumstances and at different times each of them sheltered their favorites among the military units at their command. They did so because of varying combinations of elitism, activism, romanticism, and crisis.

But a price was paid—not only in Israel, Britain, and America, but in France, French Algeria, and other places as well. The price included a certain amount of military glorification, the blurring of the meaning of and relationship between peace and war, the demoralization of regular units, the misuse of elite units, the weakening if not outright disregard of the chain of command, a decrease in discipline, and an increase in atrocities.

As in any book, there are some contradictions here. For example, Cohen believes that elite units "can [and by implication do] damage various aspects of a country's security" (pp. 27–28). At the same time, he believes that as specialists they exemplify "a productive military division of labor," are useful military laboratories for new tactics, trends, and tasks, and may be "nurseries" for new leadership (pp. 30–35).

As for the specific management suggestions I mentioned earlier, Cohen suggests (1) restricting the size and role of elite units, (2) cutting their institutional autonomy to the barest minimum, (3) discouraging the publicity that has usually surrounded them, and (4) above all, restraining somehow the penchant for some politicians to play at being soldiers. "Democratic politicians," he concludes, "should resist the temptation offered by elite units. Without such restraint the defense of a modern democracy can only suffer" (p. 102).

From "internal evidence," as historians put it, it is obvious that Cohen is a relatively new member (B.A., 1977) of the Fraternity of Scholars. But he thinks clearly, writes concisely, and makes the reader ponder. What more can we ask from a scholarly work?

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Metropolitan Latin America: The Challenge and the Response (Latin American Urban Research, Vol. 6). Edited by Wayne A. Cornelius and Robert V. Kemper. (Beverly Hills, Calif.: Sage, 1978. Pp. 346. $18.50, cloth; $7.95, paper.)

If it can be said that the decade of the 1960s was one in which Latin American urban research was in its infancy, then it is fair to claim that by the close of the 1970s there have been strong signs of maturation. In no small measure the emergence of sophisticated work in this area may be attributed to Francine F. Rabinovitz and Felicity M. Trueblood, who initiated the Sage Latin American Urban Research series. The present volume, the sixth and last in the series, reflects just how far we have come, and also shows how far we still need to go.

Wayne A. Cornelius, co-editor of two previous volumes in the series, along with Robert V. Kemper, focus their attention in this volume on nine Latin American cities, the problems they face, and the successes and failures each has had in coping with those problems. The articles follow a similar format, introducing the reader to a brief historical overview followed by succinct descriptions of the challenges which the cities face and the responses offered by the government. The editors are to be congratulated for their obviously successful efforts at maintaining the uniformity of presentation, and for their ability to include so much factual material in so little space. The bibliography of recent urban studies on Latin America, 1974–1976, updates the two earlier compilations included in volumes 2 and 4 of the series. It is to be hoped that even though the series is coming to an end, the very helpful bibliography could be continued, perhaps included in the publisher's *Urban Affairs Quarterly*.

The volume includes papers on most of Latin America's major cities: Bogotá (Allan Gilbert), Caracas (David J. Myers), Guadalajara (John Walton), Guayaquil (Richard J. Moore), Lima (Henry A. Dietz), Medellín (David W. Dent), Mexico City (Gustavo Garza and Martha Schteingart), Port-au-Prince (Simon M. Fass), and Rio de Janeiro (David M. Vetter and Ana Maria Brasileiro). Unfortunately, for reasons of space, the editors did not include an article on Buenos Aires, and a contribution on Havana did not materialize. Nevertheless, the coverage is certainly broad enough to satisfy the requirements as a solid text for a graduate level course on Latin American urbanization. Undergraduate students might find the wealth of data a bit hard to assimilate, and might better profit from reading two or three selections from the volume coupled with other essays in earlier volumes.

My disappointment with this volume arises from my unfulfilled expectation that, as the final volume in the series, it would make an attempt at theorizing and theory testing. It would appear that enough research has now emerged that such an attempt can be expected. Indeed, some recent monographs on urban Latin America have attempted to do precisely that. Unfortunately, this volume largely restricts itself to descriptive case studies. Cornelius, in his introductory essay, states that the studies in the earlier volumes "provided a fragmented and incomplete view of the metropolitan phenomenon, making systematic comparative analysis difficult" (p. 7). While each of the essays does provide a comprehensive view of the city treated, the comparative analysis is left to the reader. Indeed, Cornelius states that "the contributors were urged to resist the temptation to make detailed comparisons between the metropolitan areas examined in their case studies and other Latin American cities" (p. 9). This limitation is unfortunate, one which restricts the book's utility. John Walton's essay on Guadalajara is perhaps the one most sensitive to the question of theory, but as he emphasizes in the conclusion of his article (p. 48), a case study is the wrong vehicle for assessing theory.

Finally, the reader can heartily agree with Cornelius' conclusion that "The thrust of the evidence presented in this volume is that the degree of concentration in many Latin American countries today is clearly excessive. . ." (p. 9), but isn't that a conclusion which has been well known for some time? Despite these shortcomings, this is a book which adds incrementally to our knowledge and is to be recommended to all urbanologists.

MITCHELL A. SELIGSON

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The Resurgence of Class Conflict in Western Europe Since 1968, Vol. I: National Studies; and Vol. II: Comparative Analysis. Edited by Colin Crouch and Alessandro Pizzorno. (New York: Holmes and Meier, 1978. Pp. xxiv + 349; xxi + 334. $33.75, each.)

Major studies of industrial conflict have become a rarity in the American social science literature. The topic itself still attracts attention, but collections of essays on the vast scale of *Industrial Conflict* (1954), edited by Arthur Kornhauser, Robert Dubin, and Arthur Ross, or broadly conceived monographic studies, such as *Changing Patterns of Industrial Conflict* (1960) by Arthur Ross and Paul Hartman, seem to be a thing of the past. Most of the recent studies have eschewed ambitious generalizations, however tentative, in favor of more limited horizons, often explored with formidable quantitative research tools.

This work, an inter-European collaboration, follows the older model. Its massive two volumes contain 18 chapters prepared by 28 authors, mostly sociologists attached to research institutes and universities in Western Europe. Their inspiration was a shared interest in the increased workplace militancy which from the late 1960s on became, together with decentralizing trends in bargaining structures, characteristic of labor-management relations in most European countries. Subsequently the group's focus widened to encompass the study of industrial conflict at all levels of the industrial relations system, including the role of the state, the impact of the business cycle, and in some countries such as France and Italy the influence of changing ideologies and political alignments.

The first volume contains six country studies (Belgium, France, Italy, the Netherlands, Britain, and West Germany), together with an introductory essay on strike statistics in which Michael Shalev appropriately emphasizes the limitations of most such data. By and large the country studies follow a common scheme of organization in which the major subcategories are (1) economic, political, and social contexts; (2) new developments in the configuration of industrial conflict; (3) new demands put forward by workers; and (4) the changing nature of industrial relations systems. For comparative purposes, of course, a common format is highly desirable, but this particular sequence imposes a heavy burden on the untutored reader. Anyone not familiar with a particular national industrial relations system and its institutions (the organizations of unions and employers, the agencies of the state, the structure of collective bargaining, the role of law, etc.) will probably have considerable difficulty following the sometimes very intricate discussion of new forms of conflict and new kinds of demands without an introduction to the functioning of the industrial relations system.

The author of the Belgian chapter, Michel Molitor, disregarded the model outline and reviewed Belgium's industrial relations system before analyzing recent trends in industrial conflict. As a result his contribution is among the more lucid. Yet, like several other contributors he labors hard to demonstrate the existence of a resurgent class conflict and, I think, ultimately fails. Colin Crouch's review of British strike experience is by a wide margin the most polished, and despite problems of internal organization is probably most accessible to American readers. The chapter on France by Pierre Dubois, Claude Durand, and Sabine Erbès-Seguin is repetitive, acknowledges the existence of only the two largest labor federations (CGT and CFDT), and suffers from obscure formulations, some of which may be the result of poor translation. The Italian section by Ida Regalia, Marino Regini, and Emilio Reyneri is not only surprisingly readable but also contains a fascinating account of new strike techniques—spot strikes, rolling strikes, confetti strikes, registration strikes—invented or adapted by Italian workers to try to shift the cost of partial work stoppages to employers.

There must have been a substantial time lag between submission of chapters and actual publication in 1978, as indicated by the failure of the Dutch authors, Tinie Akkermans and Peter Grotius, to mention the January 1, 1976 merger of the Dutch socialist and Catholic trade union federations, one of the most important developments of the past decade in the Netherlands. To demonstrate the resurgence of class conflict in the Netherlands was a task on which the authors wisely did not waste much effort, although they properly call attention to a general hardening of labor-management relations. All the more determined is the attempt by Walther Müller Jentsch and Hans-Joachim Sperling to show that an intensification of class conflict (in the general sense) has occurred in West Germany in spite of most union leaders' readiness to negotiate collective agreements. According to the authors, the militancy of the German work force, however "undeveloped in comparison with that reached by the class struggles in Great Britain, France and Italy" (Vol. 1, p. 297), constitutes a repudiation of the theory "spread by the bourgeois social sciences" that the German working class is socially contented and has been integrated into bourgeois society. That interpretation is, to put it mildly, not well supported. The authors' disdain for the alleged tameness of the official union leadership is shared by some of the other contributors.

The second volume contains 11 contributions that seek to explore on a more or less comparative basis new themes in industrial conflict. Several deserve special mention. Pierre Dubois summarizes recent trends in industrial disputes and finds—not too surprisingly—more radicalization, localization, and institutionalization. Richard Hyman reviews ongoing changes in occupational structures and their impact on working-class militancy. Roderick Martin compares the internal politics and administrative arrangements of German and British unions. His analysis is insightful, but its link to the theme of industrial conflict is rather distant. A similar comparison by Rainer Zoll examines German and Italian unions, though Zoll's emphasis, in contrast to Martin's, is on workplace organization and shop-level collective bargaining. Colin Crouch pulls together the findings of several country studies on the role of the state in restructuring industrial relations systems in an age increasingly characterized by corporatist elements. Frank Soskice suggests an economic interpretation of the 1968–1972 strike wave, taking special care to explain the largely unofficial character of the conflicts. Finally, in one of the more stimulating contributions, Alessandro Pizzorno proposes a theoretical idea to explain the political aspects of labor markets and their relationship to collective bargaining. He suggests that beyond the established concepts of individual and collective exchange relationships in labor markets that are designed to ensure performance and continuity of production, there also occurs a political exchange where the chief actors are the unions and the state and where the key object of exchange is the achievement or maintenance of a basic political consensus.

As in other works of this genre, there are wide gradations in quality which even the most meticulous editing can never quite overcome. Nevertheless one wonders whether the editors might not have been well advised to omit the weakest links, for example, the chapter on "Women and Immigrants: Marginal Workers?"—all the more so when its relevance to the work as a whole is tenuous. One should also ask whether more competent translations could not have been commissioned for some of the 11 chapters that were originally prepared in a language other than English. Though most are at least adequately rendered, and a few have benefited from very skillful attention, too many passages remain that after several readings still refuse to yield up their meaning. For example:

> Calling into question as they do a system of negotiation between capital and labour that is based on the buying and selling of skill-level and in which they can have no part, unqualified workers reveal by their actions the basis of the prevailing system of production on *lack* of skill. The worker is plainly reduced to a role as a mere force of labour over and above any ideological considerations the aim of which is to value work as a means of allowing the worker to have some control over his own exploitation (Vol. 2, p. 71, italics in original).

Admittedly it is not always clear whether such obscurantism is chiefly the responsibility of the authors or the translators.

One should try to end on a positive note. For a few years some of the chapters in the first volume may be useful for reference purposes. The second volume contains several essays that may stimulate further research and discussion. Social science libraries ought to acquire the work, provided they can afford it. Apparently a Hong Kong printing job is no guarantee of reasonable pricing.

JOHN P. WINDMULLER

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The Army and Politics in Indonesia. By Harold Crouch. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 377. $18.50.)

In 1965 the Republic of Indonesia was almost torn asunder by an attempted coup. In the reaction to that event over 100,000 people were killed and President Sukarno was discredited along with the institutions and policies of "Guided Democracy." Faced with massive political violence and severe economic and social dislocation throughout the country, the Indonesian army under General Suharto became the paramount force in the affairs of the archipelago. In his book, *The Army and Politics in Indonesia,* Harold Crouch, of the National University of Malaysia, effectively chronicles the military's rise to power and ably evaluates the results of the institutionalization of army rule over the past decades. His research, based on extensive field work, will not only provide the country specialist with new material but will also be invaluable as a case study for comparative scholars interested in the role of the military in the states of the transitional area.

Crouch notes that as a result of its involvement in the struggle for independence there was an "absence of an apolitical military tradition" among the officer corps (p. 25). Yet, the political roots of the military were tempered by the "Middle Way" concept of General Abdul Haris Nasution "according to which the army would neither seek to take over the government or remain politically inactive" (p. 1). This reluctance to assume power directly was further conditioned by the divisions within the military "based on personal, regional, divisional and political loyalties" (p. 28), the preeminent position of President Sukarno and the fragmentation and diversity within an ineffectual political process. Therefore, even during the uneasy cooperation that characterized the united front during the heyday of "Guided Democracy," the army was reluctant to directly challenge the growing power of the Communist party of Indonesia. It was only when the attempted coup took place on October 1, 1965 that the military carefully filled a vacuum that would be created by the destruction of the P.K.I. and the loss of President Sukarno's base of support.

In his chapter on the coup attempt, Crouch clearly presents the different interpretations of that watershed in contemporary Indonesian history. He ably gives a balanced presentation and tightly employs the extensive documentation that is now available for scholars of the crucial period. Nevertheless, while one can appreciate Crouch's understandable caution in assessing what transpired, it would have been interesting if he had presented more directly his own learned judgment on what took place. His excellent research provides background material sufficient to extend beyond primarily recounting the conflicting versions of the coup attempt.

The author masterfully describes the army's protracted and successful attempt to strip away President Sukarno's sources of support and legitimacy. The ability of General Suharto to neutralize opposition within his own ranks on one hand, and to render "Bung Karno" powerless on the other, is a classic illustration of the indirection that can be found in much of Indonesian society and politics. Crouch also is very successful at showing how the politically astute Suharto skillfully dominated the surviving political parties by offering them "a role in the system" while at the same time taking steps "to ensure that they were headed by amenable leaders prepared to cooperate closely with the government (p. 234).

The institutionalization of army rule is further explained by the author's excellent analysis of the army's system of "unconventional financing," where "selected officers at all levels were placed in positions where they could raise funds on behalf of the army" (p. 275). Of course, as Crouch notes, such funding was also used for private purposes and consequently led to incidents of massive corruption involving "the interests of the military elite and the civilian bureaucrats and the business groups—both domestic and foreign—closely linked with it" (p. 303). Crouch is to be praised for practicing a form of investigative reporting that is too rarely found among political scientists in skillfully unweaving the economic machinations of a section of the contemporary elite. He aptly shows what can happen when an army which justifies its rule on the basis of eliminating corruption is itself corrupted by taking on a variety of nonmilitary functions.

In the concluding section of the book, Crouch cogently raises the question of the long-term ability of the Indonesian military to continue to rule. While the army is to be credited for bringing a needed degree of political order and economic stability to a troubled country, Crouch's following statement illustrates the uncertainty of the past, the present and the future. "Although civilian frustration was acute, there seemed to be no prospect of removing the military from control of the government" (p. 351). Yet, in the long run, if the army can not remove the sources of that "frustration," there is always the possibility of new conflict in the super-heated climate of Indonesian politics.

STEPHEN SLOAN

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Making Budgets: Public Resource Allocation. By James N. Danziger. (Beverly Hills, Calif.: Sage Publications, Sage Library of Social Research, Vol. 63, 1978. Pp. 225. $14.00, cloth; $6.95, paper.)

James Danziger's well-organized volume on budgeting in British county boroughs should help us all see the variety of ways in which budgets can be used in political analysis. Neither a methodological tour de force, nor a theoretical maze, his book tells how budgets may be analyzed in straightforward terms, points out the limitations and general arguments for each approach, and neatly uses his careful empirical studies to illuminate and bolster his admirable eclectic conclusions. His book will be of great interest to students and scholars who wish to have a compact and informed introduction to the politics of budgeting.

Citing the relevant theoretical literature and assessing its political significance in concise terms, Danziger examines the variations of the boroughs' budgets using the four main frameworks for budgetary analysis: demographic, incrementalist, organizational and process explanations. Those who like definitive conclusions might be disappointed that he concludes budget analysis needs "alternative explanations" (p. 219); however, in doing so, they will also miss the point. Budgets are an extremely rich source of information about local decision making and local democracy. They also provide diverse ways of disaggregating and testing macro-theories which the organization of the book clearly illustrates.

There has been considerable budgetary analysis of British towns and counties, the most ambitious previous attempt being Noel Boaden's *Urban Policy-Making* (Cambridge: Cambridge University Press, 1971). Contrary to the impression of more enthusiastic American budgeteers, studying local budgets in Britain has a history pre-dating our discovery of this data pool: for example, Claus Moser's and W. Scott's *British Towns* (London: Oliver and Boyd, 1961) which was written when local budget studies were only done in the United States by public finance experts. Local budgets also happened to be of immense interest to the early Victorian Benthamite reformer, Sir Edwin Chadwick, and to one of Britain's most distinguished chancellors of the exchequer, Sir George Goschen. All this means that sober reflection on what political science has added is welcome and useful. With less of an axe to grind than many who study budgets, Danziger helps us understand how much we have done and how much we have left to do.

Why budgets vary depends as much on the questions asked as on methodology or even abstract theory. Danziger's critique of the demographic explanations (pp. 82–83) will be welcomed by many for he shows how it represents a rather uncritical superimposition of sociological thinking on political analysis. The chapter on variations in budget allocations shows mastery of statistical skills and points to the need for further disaggregation of budget decisions. Whether this route will yield surprising or very illuminating results remains in question, even with Danziger's analysis. One wonders whether finding more statistical variation as one breaks down major services into their components (p. 63) or deals with smaller budget items (p. 75) might be simply an artifact of the method. The more surprising finding would be to have *less* variation as we subdivide budgets into smaller amounts. Whatever may be the results, Danziger's use of simple measures like the coefficient of variation seems promising if one wants to capture internal variations of decision making rather than the tried, tested and over-applied correlational techniques which blur internal variation.

Perhaps the most valuable chapter deals with organizational explanations which have on the whole been neglected by political science. Here Danziger displays his own sensitivity to the internal workings of British local government and his mastery of more complex methods. Many can do these things separately, but bringing them together is ingenious and rare. He does this by simply describing the variety of local budget processes in the four towns he has studied in more detail. Alert to the limitations of more complex mathematical models, he makes a careful evaluation of the Cyert-March simulation of bargaining under conditions of uncertainty. He points out (p. 157) that Crecine's model of municipal spending permits five decisions per department which are only enough to last most municipal department heads until lunchtime. Of course, simulation is only the tip of the iceberg in organizational theory so one hopes that Danziger's exploration will not be the limit of this avenue of inquiry.

A refreshing and lively book on a subject that has been sifted through many sieves is not easy to write. Danziger's firm grasp of the major theoretical distinctions surrounding budget analysis combined with a resourceful and flexible use of findings make his book valuable.

DOUGLAS E. ASHFORD

Cornell University

Revolution and the Transformation of Societies: A Comparative Study of Civilizations. By S. N. Eisenstadt. (New York: Free Press, 1978. Pp. xvi + 348. $15.95.)

This book, like much of Eisenstadt's earlier work, is very difficult and time-consuming. I cannot pretend to have mastered its scope and detail, but its general thrust seems clear enough, and I examined a few of the major arguments with some care.

The principal thesis of the study appears to be that revolution should be seen as a "special process through which social change and transformation takes place" (pp. 9, 329), rather than as a special kind of change. The "pure," "true," or "classical" revolutions, also called "modern" revolutions, were the Revolt of the Netherlands, the Great Rebellion and the Glorious Revolution in England, the American Revolution, and the French Revolution (p. 173). (It is not clear whether the Russian, Chinese, and other communist revolutions were "pure," although they appear to have been "modern.") These revolutions, however, are not treated consistently as processes: the author repeatedly refers to them as "far-reaching" changes or transformations, with particular scopes and goals (e.g., pp. 68, 173, 215, 334). Sweden, Denmark, and Switzerland, morever, were transformed as were those societies that experienced "pure" revolution without, however, going through the *process* of revolution (p. 197). Perhaps the issue is only terminological: a distinction between "revolution" (a process) and "revolutionary transformation" (a result) is made early (p. xv), but it is not pursued. We also are told (p. 9) that there are "different types" of revolution, but subsequently nothing is said about those that are not "pure."

Eisenstadt's concept of revolution, in short, is at best ambiguous. This in itself is not too serious. Although revolution might not be identical with a special process, something still could be said about the processes associated with the "far-reaching" changes normally considered revolutionary. The author says that the "structure of elites" *explains* "the dynamics of change of different societies" (p. 105). Yet he assumes that the concept of elite is precise and meaningful and, as far as I could determine, does not go beyond saying that, given the proper conditions, such as technological development, the more members of the elite there are and the more influence they have, the more likely that a revolutionary process will occur. There are, however, many apparently independent variables connected with revolutionary processes—characteristics of elite persons and conditions under which they operate, for example—so many that the study's conclusions are presented as catalogues (e.g., Ch. 7). The general theoretical relations among the items of these catalogues were most unclear.

The kind of "grand theorizing" Eisenstadt has presented is impressive, but I learned little about civilizations from it. There were too many undefined concepts, too much borrowing from tired theories, such as the "class" analysis of revolution, and too many subtle distinctions. Reflecting that the fault may have been mine rather than the author's, I looked more carefully at a specific problem in which I have some interest and background: the nature of "traditional" society—that is, the kind of society that preceded the modern and in which the revolutionary process (or change) occurred.

Whether revolution is a process or a result, it was "modern"—that is, an innovation—in the "pre-modern" society in which it occurred. The classic problem, of course, has been to account for the "pure" revolutions that occurred only in Western Europe and (marginally) in the United States, to discover something in the "pre-modern" societies of these areas that did not exist in "pre-modern" societies elsewhere. But, logically prior to this, there is the problem of specifying the nature of the "pre-modern," or as it is often called the "traditional." Eisenstadt's treatment of traditionalism deals at length with what he calls patrimonial, tribal, city-state, imperial, and imperial-feudal regimes, with some attention to exceptional city-states and tribal federations. The imperial and imperial-feudal systems include the Russian, Byzantine, and Chinese empires and Islamic and Western European civilizations.

Three chapters covering some 120 pages are devoted to a close examination, in the author's complicated manner, of "patterns of change" in these traditional societies. At the end of this section (pp. 159–61), Eisenstadt concludes that low levels of technology and organization, a limited conception of the possibilities of human innovation, and the political policy of divide and rule limited the "transformative capacities" of traditional society. I encountered this rather conventional conclusion stated in ordinary language with relief, for now I knew what I had been reading for the preceding 117 pages. When I then returned to analytic topics, such as the "structure of centers" and "center-periphery relations," and to the case-studies of specific regimes, I was more comfortable, but still I found only variations on the principal themes of traditionalism mentioned above.

It seems to me that Eisenstadt's model of traditionalism leads to the conclusion that traditionalism's basic characteristics not only limit "far-reaching" transformations but also prevent them (pp. 160–61). In other words, his analysis has precisely the weakness he criticizes in the "structural-functional approach" (pp. 26–27): stressing the interrelatedness of the components of a society—its ideology, stratification, elite members, social systems, technology, what-have-you—results in a logical construct that cannot even acknowledge "fundamental change," much less account for it. This, in my opinion, is the lesson to be learned from the present book: the comparative method, applied to "governments" or to "civilizations," is static and backward-looking. No matter how broad the scope and how refined the distinctions, revolution (as either process or result) cannot be understood in the way the author here has tried to understand it.

As a postscript, I do not believe it captious to take issue with the dust jacket: I cannot see how the book could provide "an entirely new dimension" to and an "important breakthrough" in the study of revolution, nor create "an eminently useful vantage point" from which to understand present and future civilizations.

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Political Development and Bureaucracy in Libya. Omar I. El-Fathaly, Monte Palmer and Richard Chackerian. (Lexington, Mass.: D.C. Heath, 1977. Pp. ix + 122. $12.50.)

Libya today is a unique specimen in the Middle East political aquarium. Like the states of Arabia, it remains a largely tribal-patriarchal society, endowed with great oil wealth, and just beginning to mobilize socially. But unlike the case in the Arabian states, a "modernizing" middle-class military leadership, on the Nasirite model, has made a breakthrough at the center, displacing the tribal monarchy by coup d'état, poising itself atop the bureaucracy and repressing the rudiments of party and ideological politics in the name of national unity. Thus, like its Nasirite predecessors, Libya's special problem is to bridge the gap between state and society and mobilize the population for development. But in Libya the response has taken a peculiar twist which, seeming to mix puritanical desert Islam with radical mass-organizational practices, leaves the observer baffled. This book is valuable because it gives some fascinating glimpses of these developments, and because, owing to a laudable collaboration between Libyan and Western social scientists, it brings to the analysis empirical techniques and evidence.

One survey, showing that the bulk of Libya's rural population hold strong particularistic, fatalistic and non-participant attitudes and support their traditional tribal leaders, and another survey, showing that these leaders are themselves indisposed to change and hostile to the regime's efforts to penetrate their local bastions, suggest the challenge faced by the new leadership. Its first response, essentially bureaucratic, was to replace traditionals with modernizing officials at the levers of local government, but attitude surveys show that, owing to the low priority the new officials gave to popular participation, they failed to win local political leadership. A subsequent effort to create a single party on the Egyptian model likewise foundered on excessive bureaucratization and the tenacity of traditional leadership. It was a perception of these mobilizational failures, according to the authors, which led Qaddafi to embark on his famous "cultural revolution," culminating in the erection of a new political structure called "People's Power." This is a Soviet-style pyramid of locally elected popular assemblies and committees which elect provincial and ultimately national bodies, vested in principle with powers of legislation and control of the executive. The evidence suggests that the new system has succeeded at least to the extent of recruiting a new third type of leadership, distinguishable from the traditional chiefs by their modest origins, and from the officials by anti-bureaucratic attitudes. Unfortunately, the authors do not give us attitude data on their orientation toward modernization and tradition, nor do they explain why a population evidently committed to its tribal chieftains did not elect them as their representatives.

The book could be challenged for a too-uncritical acceptance of the regime's own definition of the problem, namely that the obstacles to participation lay mainly in the traditional character of the population and the influence of tribal leaders. Owing to this approach, the authors take no cognizance of the serious doubts raised by Ruth First (*Libya: The Elusive Revolution,* Harmondsworth, England, 1974), as to the willingness or competence of the new leaders to mobilize participation. According to her, the regime has refused to allow the development of a true revolutionary party, virtually excluded politically active progressive intellectuals from a share of power, and, while calling for popular participation, "suffocates any political action or thought not initiated by the state." The authors could also have considered whether "People's Power" might be less a modern mobilization instrument than a new variant of old Islamic practices—some mixture of prophecy and patriarchal chieftainship which consults with the people, but reserves all decisions to itself and brooks no opposition. Methodologically, one can quarrel with the operationalization of several of the attitude dimensions. A more serious defect is the abominable organization and editing of the book. The chapters are badly integrated, repetitious, overlapping parallel presentations rather than a continuous and coherent argument. Chapter 3, a badly written mishmash of others, has no place in the book, and the last chapter, a rather esoteric discussion of public administration theory, seems barely related to the main thrust of the study. There are also numerous typographical and stylistic errors. Unfortunately, the format and presentation does credit neither to the authors' research nor the judgment of their editor.

RAYMOND A. HINNEBUSCH

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In Search of Identity: An Autobiography. By Anwar el-Sadat. (New York: Harper and Row, 1977. Pp. viii + 360. $12.95.)

The significance of autobiographies to political research has been widely debated by scholars. As self-portraits, autobiographies are too often designed to accentuate the positive aspects of a given leader's personal attributes and behavior. Well-written autobiographies of leaders in office could well constitute an effective means of projecting a favorable image to maximize public support; to the leaders of yesteryear these self-portraits or memoirs are a means "to set the record straight" by justifying their actions taken while in office. In either case it is safe to assume that virtually all such volumes are self-serving in varying degrees; nevertheless, they should not be dismissed as exercises in propaganda. When analyzed properly, autobiographies can be a most valuable source of data for historians and political scientists alike.

To writers of conventional history, autobiographies constitute "received" or "official" sources of fact and interpretation which are scrutinized along with other source materials in their efforts to reconstruct past environments of human activity. To students of political leadership, on the other hand, autobiographies are often indispensable as a source of data to determine the basis of elite behavior. President Sadat's *In Search of Identity* is no exception. It tells us perhaps as much about the man and his milieu as did Charles de Gaulle's memoirs and Richard Nixon's most revealing self-portrait, *Six Crises.*

Indeed, in contrast to the conventional historian, the primary concern of the student of leadership is not the validity of facts as such, but the autobiographer's personal view of specific events, personalities and developments which have affected his life since his formative years. In this sense the Sadat memoirs contain substantial information if one is concerned with identifying the components of his basic character and personality—how he orients himself toward the world environment, toward the Arab-Israeli conflict, the great powers and the sweep of history itself. In Sadat's case one can identify and trace the modalities of interaction between his character and his evolving environment and point to certain patterns of behavior which are likely to repeat themselves in his dealings with both friends and adversaries.

It follows that *In Search of Identity* is a veritable treasure chest for psycho-historians and elite theorists. However, two words of caution need be said before a serious psychological study is contemplated. First, memoirs constitute a good starting point and the need for additional data is a certainty. Second, the scholar must resist the temptation of blindly applying purely Western psychological constructs and paradigms to the study of a non-Western personage. The use of Freudian methodology, for example, would almost certainly produce scientifically untenable results. Even Erik Erikson's seminal study of Gandhi is plagued to an extent by the problem of cultural relativism. What is needed is an eclectic theoretical framework which is not psychoculturally blind to peculiarly Egyptian traits of personality and environment.

Meanwhile, what Sadat has chosen to tell us about himself confirms the impressions of outside observers. True to his deep roots in his native Munufiyyaha province, he is a most patient and careful planner and master of timing. Lacking the political "capital" of his charismatic predecessor Gamal Abd al-Nasser, Sadat possesses an unusual shrewdness, flexibility and sensitivity to the world milieu. Perhaps the most salient pattern that emerges is his tendency to take sudden dramatic initiatives calculated to surprise and shock foe and friend alike. Buffeted by problems that seem to defy quick solutions, Sadat uses dramatic exploits as a source of domestic legitimation and as a means of placing his Arab, Israeli and American counterparts on the defensive. In this sense the parallel between Sadat's dramatic moves—the Soviet exodus of 1972, the Oct. 1973 War, the Jerusalem visit—and Nasser's performance are more than striking. However, Sadat is less rhetorical and far more cautious with respect to Israel and other potential adversaries. In planning his quantum initiatives Sadat is a loner; his decision making is a very personal and private matter pursued in cool detachment through a mixture of rational analysis and intuition. His political behavior springs from a strong sense of self-confidence forged in prison and years of revolutionary activity, and from the deeply held convictions of a proud Egyptian nationalist.

Ignorance of Sadat's basic character traits has led to underestimation of his potential for successful leadership by political leaders, scholars and seers alike. The Egyptian president is painfully aware that his and Egypt's capabilities have been repeatedly underestimated by Israel, the Soviet Union, the United States, and others. This he has turned to his advantage by unleashing "shock effect" initiatives when least expected.

Readers of differing perspectives will find a great deal in these memoirs which might be considered objectionable. Indeed, there are a number of gaps in Sadat's account which betray the Egyptian leader's blind spots and shortcomings. A case in point is Sadat's general unconcern with the economic situation at home which reflects his order of priorities the first of which is Arab-Israeli peacemaking. Meanwhile, the home front is bound to remain a vulnerable area for the Egyptian president in the foreseeable future. Another possible danger is the tendency to achieve a world-wide impact as peacemaker too zealously, although Sadat is too much of a pragmatic politician to engage in messianic acrobatics.

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Black Power in South Africa: The Evolution of an Ideology. By Gail M. Gerhart. (Berkeley: University of California Press, 1978. Pp. xi + 364. $14.50.)

Over the years black political aspirations in South Africa have constantly been blocked by the white system and Africans have only marginally participated in the realm of white politics. But this has never meant that Africans have not engaged in politics. Within the nationalist movements, political recruitment, political socialization, and the aggregation and articulation of interests have all occurred with increasing intensity. Despite harsh and overwhelming difficulties and the hostility of the white system, Africans have financed and organized nationalist movements, held meetings, disseminated publications, communicated with branch organizations, and traveled across the country often at grave personal cost. It is possible to trace a linear development in African politics from the African National Congress (ANC) in 1912, whose leaders sought acceptance by the white system for an educated elite, to the violence and militancy of the Soweto generation of the 1970s. Indeed, the increasing violence, both physical and psychological, done to Africans by the white system has been reflected in a parallel frustration and escalation of violence by some blacks.

Many questions have arisen concerning the politicization of Africans in South Africa. How deep have the differences been between the ANC and the more militant Pan-Africanist Congress (PAC)? What factors have led some Africans to commit themselves to a violent rather than constitutional struggle? Why have some factions accepted the involvement of white liberals and others rejected it? What would be the place of whites in a future black state? What means would be acceptable for the achievement of black political goals? What relationship has existed between the black urban elite and the masses? And finally, to what extent are factors such as age, personality, class, and personal experience relevant to the differing strands of African political thought?

Gail Gerhart, in an important study, *Black Power in South Africa: The Evolution of an Ideology*, addresses many of these fundamental questions. She looks at the divisions of black leadership and the evolution of black consciousness in the 1940s and 1950s by leaders such as Anton Lambede, Jordan Ngubane and Asby Peter Mda. These were political leaders and theorists who were concerned with the psychological damage done to Africans in a white-dominated society and who sought to inspire the masses with a sense of their own dignity which they believed in turn might lead to political action.

Gerhart correctly traces and analyzes the evolution of this increasingly significant strain of African political thought and examines the role of the African urban intelligentsia in the process. The distinction she draws between orthodox or "Black Power" nationalism and what might be called the *liberal or non-racial nationalism of the ANC* is vital to our understanding of African politics in the period since the Second World War. Orthodox African nationalism (also sometimes called "exclusivist nationalism") defines South Africa as a country belonging to Africans by right of first possession, and on the grounds that they are the great majority of the population. Furthermore, for the orthodox nationalist, whites are guests in the African house permitted to remain in Africa only on terms set down by their indigenous hosts. Non-racial nationalism has aimed at a pluralistic sharing of power, while orthodox nationalism has tended towards a "winner-take-all" view, with Africans winning power to control the state and, ultimately, the economy through the state. Orthodox nationalism for the most part leaves the future fate of whites unclear, apart from suggesting that they should somehow merge themselves into the predominant African population.

The overriding question is whether these strands of thought and action constitute a fully fledged ideology or even an emerging ideology. More accurately, at this point, they are perhaps expressions of militancy. Ideology implies an all-encompassing integrated system of social and political action and thought, and as such differs from the more restricted notion of militancy. Violence and revolutionary strategies do not in themselves constitute an ideology. While there are clearly ideological elements in the thinking of more militant African leaders, it is doubtful whether an ideology has emerged. Most Africans continue to see the struggle as one against *apartheid,* but large numbers still differ with the more militant approach to resolving the problem. An ideology should receive validation and acceptance by at least a significant segment of the masses or else it remains a theoretical concept.

Gerhart's analysis is provocative and interesting. She is aware that she has deliberately overdrawn the ideological positions in order to make distinctions clear, and her device succeeds. Her polarized position emphasizes the roots of militancy in South Africa and points to the increasing possibility of violent upheaval in that country.

Without doubt this is one of the most important books to emerge in Southern African studies in the past several years. It is an interesting fusion of theory, analysis and historical information. The book not only helps us to understand important figures of the past, but also the relevance of contemporary leaders such as the late Steve Biko and Robert Sobukwe. Perhaps the greatest importance of *Black Power in South Africa* is that it provides us with insights into a future in which black power and white intransigence might confront each other.

PATRICK O'MEARA

Indiana University

Health Insurance Bargaining: Foreign Lessons for Americans. By William A. Glaser. (New York: Gardner Press, 1978. Pp. xiii + 265. $18.00.)

That this book can't be given unqualified praise is a reflection more on the state of the discipline than on the quality of this work. Glaser defines national health insurance quite conventionally as a method for organizing a country's spending for doctors, hospitals and other providers of health services for all citizens. Reasoning that the United States is the only industrial democracy without a broad-gauged public health insurance plan and that we may soon adopt a comprehensive program, Glaser concludes that we should learn from the experience of other nations.

Approximately 80 percent of the book is given to a country-by-country description of the national health insurance schemes in Canada, France, Belgium, the Netherlands, the Federal Republic of Germany, Switzerland, Sweden and Great Britain. In each case, Glaser focuses on describing the structure of the health insurance system with particular stress on the machinery used to determine physicians' fees. Except for Great Britain, where National Health Service doctors are direct public employees, sick funds similar to Blue Cross/Blue Shield receive and disburse funds for health services, using a fee schedule developed by negotiation between the largest medical association and a semi-autonomous public agency. The character and structure of these negotiations is described in some detail and the strengths and weaknesses of each national health insurance system are listed by the author. Lamentably, the book contains no comparative economic statistics. A comparative analysis of the median income of doctors and the percent of GNP allocated to health services in each country surveyed might be revealing and certainly would have increased the utility of this book.

European experience may not be a particularly good predictor of the pace or direction of the American move toward national health insurance. When national health insurance was phased-in abroad, occupational groups were covered successively. Among the national health bills pending in the Congress, only the Kennedy bill proposes a full-blown, European-style national health insurance plan. Competing bills, according to Glaser, "do not constitute national health insurance at all and are methods of . . . expanding existing nonprofit and for-profit insurance" (p. 210). The competing bills maintain the "usual and reasonable charge" method for determining the doctor's fee and expand medicare health insurance to include children, the unemployed, and victims of catastrophic illness.

Implementation of national health insurance in the countries surveyed commonly produced certain policy problems which might be anticipated in the United States should the Congress pass the Kennedy health bill. In every case, national health insurance was based on a negotiated fee schedule rather than the "uniform and reasonable fee" system used by medicare. In most cases, introduction of national health insurance led to the development of militant unionism among hospital doctors, interns, and residents and consequent large and unanticipated increases in the cost of hospital care. To complicate fiscal problems, national health insurance costs not covered by employer-employee contribution must be covered from general revenues. Thus, national health insurance is inherently entitlement legislation and government expenditures for national health services are uncontrollable in precisely the same sense that social security costs are uncontrollable.

The European experience, as explained by Glaser, suggests that the adoption of national health insurance in the United States will occasion a radical restructuring of relationships within the medical fraternity and between the medical fraternity and the government. Adopting national health insurance will also signal the fiscal and organizational expansion of the federal government into a new and important area of activity. *Health Insurance Bargaining* has made an important contribution to the developing literature on this timely topic.

GAYLE AVANT

Baylor University

Fascism in the Contemporary World: Ideology, Evolution, Resurgence. By Anthony James Joes. (Boulder, Colo.: Westview Press, 1978. Pp. xvi + 238. $17.50, cloth; $8.50, paper.)

Central to this book is the argument that fascist regimes similar in ideology and structure to Mussolini's in Italy constitute a logical and quite rational response to the problems of modernization in late-developing nations. Anthony James Joes views fascism not as an aberration that can be exorcised or wished away and certainly not as a phenomenon of a given time and place—i.e., Europe between the wars. Rather, he sees fascism as arising and continuing to arise as a definite reaction to the need for discipline, order, and authority in modernizing nations torn by factionalism and the possibilities of disintegration and seeking to achieve integral national development.

In this study Joes explores the similarities between past and contemporary fascisms and seeks to explain the circumstances in which fascism develops, to whom it is attractive, and what its recent reemergence signifies. In the first part of the book he examines the development of fascism in Italy and uses the Mussolini regime as his prototype. He arrives at a definition of fascism that stresses the following factors: nationalism, repudiation of parliamentary liberalism, statism, productionism, corporatism, authoritarianism, and elitism. Armed with this model, Joes proceeds in part 2 to an examination of fascism in the developing world. He finds characteristics of and tendencies toward his fascist model in regimes as varied as Meiji Japan, Perón's Argentina, Nkrumah's Ghana, Nasser's Egypt, and the present Brazilian and Peruvian regimes.

Joes writes in the provocative manner of A. James Gregor, whose writings on fascism he quotes often and who has added a foreword to the book, and his analysis contributes many insights. He argues forcefully for understanding the complex multicausality of Italian fascism rather than relying on any single, monocausal explanation. He develops some fascinating parallels between the Italian situation prior to Mussolini's coming to power (delayed and dependent economic growth and industrialization, incomplete national integration, unsuccessful experimentation with Western liberalism) and the frustrations felt and the search for new, alternative models experienced by many developing nations today. He concludes provocatively that for Third World leaders fascism represents a more widespread and attractive response to the wrenching problems of modernization than does either liberalism or socialism.

Although many of Joes' arguments and analyses are interesting and persuasive, the book also gives rise to a number of problems. Probably the foremost is the use of the term "fascism." First, many students of fascism in Italy would define it quite differently from Joes, perhaps stressing its totalitarian nature, its secret police, its racial persecution, its "behemoth" qualities, as distinct from the rather mild authoritarianism described in this book. Second, Joes has a problem in his effort to apply the fascist model to the present-day developing nations. On the one hand, there is so much emotive content to the term "fascism" that its use as a precise analytical tool has been almost entirely lost. On the other, it strains credulity to use the term in reference to such varied persons and regimes as Nasser, Perón, Vargas, Nkrumah, Senghor, Nyerere, and the Brazilian and Peruvian militaries, a number of whom consider themselves syndicalists, socialists, or perhaps corporativists, but certainly not fascists.

In using the fascist label to describe such diverse regimes, Joes has both cast his explanatory net too wide and got himself in a mesh of troubles. If he had focused, as other scholars are presently doing, on the nationalist, statist, corporatist, authoritarian, developmentalist, and elitist nature of these systems, both the Italian prototype and the Third World parallels, he would have been on much more analytically surer—and safer—ground. It is the use of the term fascism and its broad sweep—too broad—that gets Joes in difficulties, as it has Gregor. One senses that Joes recognized this, albeit too late, since he has a fascinating appendix called "A Closer Look at Corporatism" which is far less emotionally charged than is his fascism analysis. Had Joes proceeded to examine the Mussolini system and those Third World nations he discusses in terms of the more neutral set of characteristics he himself had identified, he would have produced a sounder analysis. As it is, one must pick through the analysis for its useful insights since the main theme is not argued convincingly.

HOWARD J. WIARDA

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Divided Loyalties: British Regional Assertion and European Integration. Edited by Martin Kolinsky. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. viii + 216. $19.25.)

How can the United Kingdom accommodate two apparently opposing forces of radical political change, European integration and the devolution of power to regional assemblies? The question could as well be asked of Belgium, France, Italy or Spain, each of which is experiencing the simultaneous stresses imposed by integration and sub-state nationalism. These came to a head in Britain after the general election of October 1974 which saw the Scottish Nationalist party emerge as the second largest in Scotland and *Plaid Cymru* make important gains in Wales. Because of internal party divisions the Labour government had already committed itself to reviewing British membership in the EEC and this was put to a referendum and approved in June 1975. In the fall of 1976 proposals to establish regional assemblies in Scotland and Wales were introduced in Parliament.

This book, a collaborative effort by 12 British scholars, does not take us far beyond the question with which we started—how can the two policies be accommodated?—but it does make a useful contribution by setting the scene and clarifying some of the problems. It describes the attitudes of the political parties and other groups and identifies three sets of relationships which have to be considered—the U.K. and the EEC; the U.K. and the regions, Scotland, Wales and Northern Ireland; and the EEC and the regions, a particularly interesting new topic. Edward Moxon-Browne, for example, finds that direct relations between Northern Ireland and the EEC are bringing a new dimension to the Irish problem. Martin Kolinsky discusses changes in parliamentary activity and, in the best essay in the collection, Helen Wallace deals with the constitutional and administrative problems posed by integration and devolution. She is particularly effective in placing them in a context which includes civil service and local government reforms between 1964 and 1975. The result, she argues, has been an unsettling climate of continuous administrative reorganization.

The great failure of British government in this period of great activity is summed up by Wallace when she writes, "No clear conception of a wider strategy of constitutional change has at any point been evident" (p. 91). This is also the great weakness of the book. More than a decade of feverish activity has left us with no clear sense of constitutional coherence in the United Kingdom. Kilbrandon's *Royal Commission on the Constitution,* which recommended devolution, refused to review the constitution in any fundamental sense. But for the referendum defeat last March, the U.K. might soon be governed in six different ways, Scotland by an assembly with executive and legislative powers, Wales by an assembly with only executive powers, Northern Ireland by some form of power-sharing and England by the U.K. Parliament. That Parliament would also have certain responsibilities for the U.K. as a whole and the EEC would have authority over the country as a member of the community. This would amount to six different, if overlapping, levels of authority. Furthermore, the relationships between these levels would be ambiguous, largely because of the uncertainties inherent in an unwritten constitution and the rule of parliamentary supremacy. The authors are aware of this constitutional chaos but do not address it directly or explore remedies.

This constitutional disarray has a number of explanations. First, as Wallace suggests, coherent policy making is inhibited by the internal divisions within the major parties. Second, the government has reacted to the fait accompli of the EEC and the threat of disintegrative sub-nationalism with policies which are primarily expedients rather than reasoned constitutional innovations. Third, since the days of Irish home rule, the United Kingdom has responded to challenges to the regime by trying to legislate radical changes in political institutions without radically changing the principle of parliamentary supremacy. This process continues today and is a tribute not to a flexible constitution but to an inflexible one. Fourth, the constitution and the Westminster model both operate to centralize power in the hands of the executive and the bureaucracy. Those who have power are pathologically unwilling to surrender it and one of the ironies of devolution and integration is that, as this book demonstrates, the central government has been strengthened as it acts to coordinate policies between the U.K., Europe and the regions.

These essays, and most British academic writing, share with the government and the major parties an apparent unwillingness to come to terms with the real condition of the constitution.

ALAN J. WARD

College of William and Mary

The Working Class in Welfare Capitalism: Work, Unions and Politics in Sweden. By Walter Korpi. (London and Boston: Routledge & Kegan Paul, 1978. Pp. xxi + 448. $25.00.)

Since Marquis Childs' *Sweden: The Middle Way* (New Haven: Yale University Press, 1936), Sweden has held quite a fascination for Americans. Here was a powerful labor movement which successfully engineered an alternative to the fascist and communist polarization of the 1930s with its pursuit of welfare state Keyneseanism. The result was, or so it seemed, class harmony. As most advanced nations followed step in the postwar era, the class harmony thesis was generalized to all affluent societies in the form of "end of ideology" or "logic of industrialism" theories. Reality followed by social research has generally invalidated these optimistic assumptions but, unfortunately, research has not yet given us much information on what, alternatively, the real nature of modern class politics is. Walter Korpi, one of the most interesting Swedish sociologists, gives this old debate a remarkable twist and offers a new perspective on working-class politics which demands serious attention.

In his central thesis Korpi delivers a simultaneous attack on pluralism and simplistic Marxism. He holds that the Swedish social democratic welfare state (it is unclear whether we can generalize beyond Sweden) has not made workers embrace bourgeois society, nor has it arrested the formation of a socialist consciousness.

Korpi understands class conflict in terms of a political process model which stipulates that the nature of conflict varies with the distribution of power resources in civil and political society. The militancy of French workers, for example, may reflect their lack of power resources just as the tranquility of Swedish industrial relations may testify to the vast power of working-class organizations. The accumulation of class power is not merely the immediate result of inherent tendencies in capitalism's maturation, but is also conditioned by the political and economic achievements of the labor movement itself. Power resources flow from the level of working-class solidarity which, in turn, depends on the types of reforms and class organization which have evolved. Moreover, the growth of power accelerates the drive towards democratic socialism because additional power resources will be brought to bear on lingering social inequalities.

The bulk of Korpi's monograph subjects this general theory to the empirical test. Following a brief profile of the history of the Swedish labor movement, Korpi marches us through a detailed analysis of working-class attitudes based on a sample of Swedish metal workers. He covers an amazing variety of issues such as extrinsic-intrinsic job satisfaction, shop floor conditions, union leadership and structure, worker influence, and so on. The treatment is methodologically excellent, but substantively a little tedious and at times the variables appear rather peripheral to the book's central argument. The sample is large and often the coefficients reported are not especially strong. The cumulative evidence, however, manages to destroy conventional myths of a prevailing economism and narrow instrumentalist working-class consciousness. The workers do appear to view themselves as part of a collectivity with a strong sense of class solidarity; such is true of Swedish metal workers, at any rate.

About half of the book is devoted to the metal worker sample, and that seems extravagant. Korpi introduces one new variable after another and they cover an extraordinarily broad range. In effect, the reader is overloaded with attitudinal dimensions and their precise theoretical relevance is not always clearly stated. A few carefully chosen relationships would, I believe, have convinced us equally effectively.

But the chief problem with metal worker attitudes is that they speak only indirectly to the critical question of class politics and power resources. The utility of Korpi's findings is somewhat limited when, in the final chapters, Korpi returns to these issues. Here the power position of the labor movement, the Social Democratic party in particular, is examined in light of its class support, the emergent alliance between manual and white-collar workers, and the programmatic shift towards economic democracy. The Social Democrats' traditional dependence on private economic growth is seen as conditioned by their lack of sufficient power. The adoption of the Meidner plan for economic democracy, however, attests to the radical shifts occurring in the Social Democrats' underlying power base. The hypothesis offered is that these shifts are of sufficient magnitude realistically to raise the demand for socialism on the political agenda.

The possession of a decisive degree of power rests on a number of key variables, one being the consciousness and solidarity present in the working class; secondly, the role of the manual workers' union federation—the LO; thirdly, the white-collar federation—the TCO; and finally, the Social Democratic party. The massive power of the LO, which represents about 90 percent of manual workers, is clear. It has made massive inroads into managerial prerogatives. Since the potential for economic democracy and democratic socialism is contingent on the TCO, we need to know the extent to which the white-collar workers will align themselves with LO workers and the Social Democratic party. Korpi argues that the TCO's position is converging with that of the LO and that its politics are coming closer to the Social Democratic party: this formidable coalition is already in the making. With regard to the Social Democratic party, we need to know its capacity for mobilizing an electoral majority.

The latter two factors are not convincingly argued. The party's election performance has been slipping since 1968, and it is presently in opposition. Korpi's data indicate a substantial drift of working-class votes towards the Center party on the right, and they even suggest that the Social Democrats are losing their hold over younger-generation workers. Gains among white-collar workers are insufficient to offset losses among manual workers, and with a weakening grip on the young, the party's future appears problematic. The absence of a more extensive treatment of the TCO also hampers Korpi's argument. For instance, it would be helpful to know what rank and file white-collar workers think of economic democracy.

This is one of those books which disappoints at first reading, but later begins to creep up on you and demands that you seriously rethink old, comfortable ideas. It has flaws and loose ends which certainly have nothing to do with the author's scholarly qualities, but which attest to the inevitable gap which must occur when a challenging and ambitious theory confronts insufficient data. The long attitude survey may be tedious reading, but the study as a whole is intellectually exciting and provocative. There are not many books on this theme of equal stature. Also, it is easily the best treatment of Swedish class politics available in English or, for that matter, in Swedish. It demonstrates just how far Sweden—and the study of politics—has developed since Marquis Childs. Korpi may not exactly prove that the working class remains the "gravedigger of capitalism," but he does make a strong case for why we should study this issue in today's welfare state.

GÖSTA ESPING-ANDERSEN

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Eurocommunism: A New Kind of Communism? By Annie Kriegel. (Stanford, Calif.: Hoover Institution, 1978. Pp. xiii + 131. $12.00.)

Annie Kriegel wrote this short essay nearly two years ago in response to a valid question posed by many at that time: is Eurocommunism a new kind of communism? Does it represent a potential challenge to Soviet leadership of the communist world similar to the challenges of the Yugoslav and Chinese communists? To answer these questions, Kriegel first looks at three leading definitions of the Eurocommunist movement. To those who see it as *a new variety of communism that combines socialism and freedom*, Kriegel responds that for all the references to civil rights, bourgeois freedoms, pluralism, renunciation of the dictatorship of the proletariat, etc., the Eurocommunist parties continue to practice a democratic centralism which badly undermines their new-found commitment to tolerance of contending ideas and the democratic process. The second possible definition of Eurocommunism, according to Kriegel, is that it is *a regional strategy for the conquest of state power.* Given its limitation to France, Spain and Italy, we probably ought then to call it SemiSouthern-Eurocommunism.

But more importantly, Kriegel points out that on major questions, these three parties are by no means unanimous. Thus she contrasts the timidity of PCF and PCI criticism of the Soviet Union with Carillo and the PCE's more aggressive stance. On foreign policy questions, such as NATO and European construction, she contrasts the views of the PCI and PCE versus those of the PCF. On domestic programs she groups the PCF and PCE versus the PCI. These shifting coalitions lead Kriegel to stress the third definition of Eurocommunism—*national communism.* Thus Eurocommunism represents a "temptation" on the part of independent Western European Communist parties to "escape from the ruins of Stalinism" (p. xi) in their own individual ways. Nevertheless, Kriegel finds the temptation hardly overwhelming and remains unimpressed with the Eurocommunists' efforts to distance themselves from their Stalinist pasts or from the Soviet Union.

Kriegel goes on to note that each of these definitions contains some truth, though always qualified. What then, she asks, are the obstacles to the full development of the "as yet mostly undifferentiated embryo of Eurocommunism?" (p. 54) First, Soviet hostility; second, the fact that the Eurocommunist label has not appreciably increased the voting strength of those parties; third, the lack of room for maneuver. That is, the Eurocommunists are not alone on the left—close by are the democratic socialists, the *gauchistes,* the Neo-Marxists, etc. So the Eurocommunists are in limbo, because "the alloy of verbal radicalism, practical reformism, and strategic nationalism, besides being likely to break down because of the inherent incompatibility of its components, is not capable of providing its own legitimacy" (p. 58).

There are three scenarios open to Eurocommunist development, according to Kriegel. Return to traditional orthodoxy, rupture with the Soviet Union, continuation of the present—confused—course. Kriegel concludes that this last scenario is the most likely one. After all, she argues, when compared to the communist world's attention to Asia, Africa, and the Middle East, Eurocommunism is a peripheral, not a major, communist development.

Kriegel's book was one of the first to raise interesting questions and outline options with regard to Eurocommunism—yet that is precisely the problem. Written originally in French in the spring of 1977, published in English in the summer of 1978, the book is now outdated, superceded. Of what use, for example, is the final third of a book which speculates on what would happen once the united left in France won the March 1978 legislative elections? In fact, the PCF decision to sabotage that united left victory has shed new light on Eurocommunist development. If the Eurocommunists are really committed to the (democratic) emancipation of the European working class (their logical aim, according to Kriegel), why do they favor governments of national union with conservative or centrist elements, rather than union of the left governments? The French Communist party courts the Gaullists while vehemently criticizing the Socialists; the Italian Communist party seeks power with Christian Democrats rather than with Socialists; the Spanish Communist party speaks favorably of the center-right government in Spain. Is it possible the real communality of interests among Eurocommunists is now the forestalling, the undermining of the democratic socialists' growing "Europeanization" (cooperation at the EEC level, especially after the direct elections in June 1979) and "internationalization" (in a reinvigorated Socialist International)?

NANCY LIEBER

Paris

Women in Soviet Society: Equality, Development and Social Change. By Gail Warshofsky Lapidus. (Berkeley: University of California Press, 1978. Pp. x + 381. $17.50.)

Gail Lapidus' work on women in the USSR is a masterful, comprehensive, well-researched book which will be frequently cited by scholars and students of Soviet and women's studies for the foreseeable future. She demonstrates a thorough grasp of relevant Soviet and American literature, skillfully applying the conclusions and insights of other writers to her presentation of data and to her own analysis. Her knowledge of Russian is effectively reflected in the diversity of both well-known and little-known Soviet sources used. The book is enhanced by the extensive list of sources in the bibliography and footnotes. The overall approach and coverage of the work also testify to her credentials in sociology as well as political science.

Among the major topics covered in this lengthy work are: women in pre-revolutionary Russia; a history of Soviet policies affecting the status and roles of women; overviews of women in the work force, in politics and in the family; an examination of sex roles; and finally, general conclusions. Most of the topics are treated in considerable detail, even though study of some issues is handicapped by the paucity of available Soviet data. The book purports to study the question of women more or less as a case study in the politics of equality, especially focusing on the impact which Soviet-style modernization has had on equality. The dual theme is somewhat lost in the development of the various topics, although the author returns to it in the concluding chapter. In so many areas of life Soviet women have clearly not achieved equality, but a sharper, more concerted pursuit of this dual theme in each major subdivision would have been possible.

In launching her study, Lapidus examines first the nineteenth-century women's movement in the West generally as well as in Russia. Perhaps more space could have been devoted to the movement in Russia and less to general nineteenth-century developments. Her treatment of Marx and the women's issue raises as many questions as it answers. While one can agree with her that the women's question was never a central one to Marx, her analysis of Marx's positions is sometimes unclear. Her treatment of the reforms adopted in the USSR in the 1920s and of the changing policies of the 1930s is especially noteworthy. The chapter, "Enabling Conditions: Affirmative Action, Soviet Style," presents an excellent overview of existing laws and of areas of strength and weakness in Soviet policy.

The work definitely belongs in every college and university library and in the personal collection of scholars interested in Soviet domestic policy. It makes a major contribution both to policy studies of women and of the USSR. The sophisticated treatment of the material and the author's presumption that the reader is equipped with considerable knowledge of the Soviet Union suggest that the work can best be used by the professional scholar or advanced student. This work is not intended for the casual lay reader.

Despite a preponderance of positive features which deserve recognition and commendation, the work has some weaknesses. Although Lapidus writes very well, at times her points are ponderously made or lost in a maze of complex, slightly meandering arguments. In several places she suggests there may be important implications arising from a particular trend or policy, but fails to examine them. The lack of follow-through on some problems studied may be a by-product of such a far-reaching study, but does occasionally leave one frustrated. For example, on p. 143 Lapidus states that the fact that girls outnumber boys in secondary education may have significant implications for subsequent occupational roles but does not attempt to explore further. In the chapter on work, she points out that, from the 1940s to the 1960s, many women entered the field of engineering, a prestigious occupation in the USSR from which many promotions flow (pp. 182–83). Yet few women engineers are in top administrative or managerial positions. Is the "time lag," or late entry of women into this field, the single most important explanation, as suggested in the text? Again in the chapter on politics, she cites the "time lag" as a factor in the failure of women to reach the higher echelons of power in significant numbers. The Soviet system is now over 60 years old, and the commitment to women's rights was part of the revolutionary thrust. Yet, after six decades few women make it to the top in politics or industry. In at least two sections of the book, she alludes to the increasing "privatization" of life in the USSR but fails to give a thorough analysis of its possible impact on the status and roles of women.

In the penultimate chapter, Lapidus focuses on major problems affecting women which exist in the USSR today. Perhaps too much attention is devoted to population policy while relatively little is given to some other cogent issues. In the concluding chapter, the author attempts to put the question of sexual equality in comparative perspective. Coverage of this topic could be expanded considerably both here and elsewhere in the work.

On balance, the work is excellent and can be highly recommended to political scientists and other social scientists as a landmark study in the field.

NORMA C. NOONAN

Augsburg College

The Korean Workers' Party: A Short History. By Chong-Sik Lee. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xiv + 167. $5.95, paper.)

This monograph is the first in the Hoover Institution Press series on the histories of the 16 ruling communist parties. Chong-Sik Lee is the author of the widely read *The Politics of Korean Nationalism* and the award-winning *Communism in Korea,* the latter written in collaboration with Robert A. Scalapino.

Casual students of Korean affairs and history would tend to equate the communist movement in Korea with the indefatigable Kim Il-sŏng and his Korean Workers' party, which is the official name for the North Korean communist party. Kim's successful elimination of his rival factions in the late 1950s and his highly propagandized doctrine of "Chuch'e," or "self-reliance" (not "the foundation of action," as this author would have it), have had the effect of encouraging that dubious equation. The establishment in more recent years of a veritable civil religion devoted to the adoration of the supreme leader and his supreme family would seem to suggest that this misconception has been carefully woven into the fabric of the North Korean belief system.

Lee's immensely knowledgeable account of the evolution of the Korean communist movement comes as a salutary corrective. It reminds the reader that there was a communist movement in Korea long before the victorious Soviet Army installed Kim as their man in 1945 and that it was, above all else, an extension of the nationalist movement whose principal objective was to liberate the country from Japanese colonial rule.

It is not surprising to learn that the early leadership of that movement was relatively uninterested in the ideological disputations which imbued the sister communist movements in the Soviet Union and China. Indeed, Lee lays major blame for the tribulations of the prewar Korean communist movement on the Comintern's wrong-headedness. Having affirmed, as it did in its 1928 "December Theses," the primacy of the anti-colonial political struggle over the class struggle aimed at an agrarian revolution, the Soviet leaders got carried away in the early thirties, so it would seem, with a mindless radicalism which demanded an all-out assault on every reformist group in Korea whose ideology and objective did not jibe with the Comintern orthodoxy, branding it as "reactionary." This posture stifled the anti-colonial struggle in Korea; but, further, it hopelessly undermined the cause of class struggle itself, which had yet to find a foothold in a society dominated by stubborn conservative traditions.

The author makes a catalogue of unfavorable circumstances under which the Korean communists had to operate prior to the liberation of 1945, and in so doing he seeks to explain their "repeated failures" (p. 29) at carrying out their twin objectives of an agrarian revolution and the overthrow of Japanese rule. He cites, in particular, the uncanny efficiency and ruthlessness of the Japanese police. But it is not clear what sort of developments the author would have required as evidence of the communists' "success." For, surely, he does not believe that the Japanese would have stood by while the Koreans carried out their revolution, much less pulled out under the pressure of a Korean "united front," however massive or determined such an effort might have been.

This reviewer's major reservation about the book, however, has to do with its treatment of the post-liberation period. Lee's analysis of the radical politics under the U.S. occupation forces and the outbreak of the war in 1950 betrays a curious lack of critical insight, at times making him appear downright naive. To be sure, it may be some time before we can synthesize our fragmentary knowledge of these subjects with any finality. Nonetheless, enough has been told in recent years to compel a serious reappraisal of much of the conventional wisdom which has been bequeathed to us as a cold war legacy. When it comes to these crucial topics, our author seems in no great hurry to revise his old views.

This book still remains a welcome addition to the literature, certain to please especially those who have an appetite for the subject matter but not the patience required for plowing through a full-blown study. Lee has given us both kinds of work: after one is finished with this volume one can always turn to Lee's larger works to sample his better efforts.

SUGWON KANG

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The Politics of the Chinese Cultural Revolution: A Case Study. By Hong Yung Lee. (Berkeley: University of California Press, 1978. Pp. xiii + 369. $16.00.)

Lee is not the first writer to pursue an interest-group approach to China's Cultural Revolution of the 1960s, but his effort is the most thorough and well-researched one. His main source of data is Red Guard newspapers, as catalogued and indexed in *A Research Guide to Red Guard Materials in the United States* (Occasional Paper, University of Michigan Center for Chinese Studies, forthcoming). Such documentary findings are soon to be augmented by the work of Stanley Rosen and others who have undertaken extensive interviewing of Cultural Revolution participants.

The interest-group approach has characteristic strengths and weaknesses. The strong side relates to the interior of the revolutionary process, that is, to the dynamics of the movement once it was under way. To quote Lee's own concise summary of his complex argument:

> The basic component of Mao's strategy of mass mobilization was to exploit the existing social contradictions by shifting his support from one group to another, according to the groups' positions on a particular issue and according to the practical needs of the moment. This strategy enabled him to supplement his diffuse power with genuine support from particular political groups, and at the same time allowed him maneuverability in his actions. He relied on the masses to criticize the elite and to break the coalition of reds and experts within the elite. But, at the same time, he continued to recognize the need for leadership from the top and actually used the organizational capability of the PLA to maintain control from the top. He mobilized the radicals to attack the power holders and to compel the privileged social groups to renounce their privileges. On the other hand, he depended on the conservative mass organizations to put pressure on the bourgeois experts and to check those with bad family backgrounds from directly challenging the Marxist class line (p. 330).

The weak side of the interest-group approach is its tendency to slight both the origins and the ultimate consequences of revolution. It is a way to make some sense of people's motivations and strategies only in the short run.

The most well-documented parts of Lee's study are hypotheses about group participation in the Cultural Revolution, such as: "The radical mass organizations were largely composed of underprivileged social groups, whereas the conservatives were heavily drawn from the better-off social groups" (p. 5). The seven "actors" to whom he gives the most explicit attention are Chairman Mao, the Cultural Revolution Small Group, the People's Liberation Army, the party organization, the government, radical mass organizations, and conservative mass organizations. These are admittedly very large aggregates, but Lee is able to draw several meaningful conclusions about them nonetheless. Hypotheses about elite-mass relations and about authority fill out his interpretation of this period. Generally, however, these are less carefully developed. Lee's points about groups are his original and important empirical contribution.

GORDON BENNETT

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Iran Under the Pahlavis. Edited by George Lenczowski. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. xxii + 550. $19.95.)

Along with many other close observers of the Iranian scene, the 12 authors of these collected essays completely failed to foresee the turmoil which engulfed Iran in 1978 and led to the downfall of the Shah. On that score alone, the authors are blameless. But all too often these essays are apologia which fail to offer even the slightest analysis of the forces which so violently erupted. With few exceptions, these pieces constitute a celebration of the Pahlavis.

Only Charles Issawi in his "The Iranian Economy 1925–1975: Fifty Years of Economic Development" warns that "In the mid-1970's Iran's growth was definitely excessive, and was straining the social fabric and administrative capacity" (p. 163). He faults the Iranian economy for failing to provide sufficient employment opportunities, failing to check inflation, and warning that Iran's growing inequality of income was "a deplorable and socially dangerous trend" (p. 165).

But with almost no other exception in over 500 pages, one would be hard pressed to recognize the same Iran whose monarch left for an "extended vacation," whose economy is in a shambles, whose population reveres a long-exiled religious leader, whose army collapsed under the immense strain of months of martial law and brief fighting, and whose formerly feared secret police remains in total disarray.

What the book demonstrates is a form of what might be labeled "Pahlavi historiography"—a focus on a very select set of aggregate statistics about Iran as an entire system or about Iran's international relations. Missing is the politics—whether it be of literature or land reform; of religion or education, of social change or planning. Who did what to whom with what effect can rarely be discovered in these pages.

In the emergence of Iran from under the Pahlavis, it seems appropriate to ask how Western scholarship became bewitched by the Pahlavis as did Western journalists and most Western governments. This is the appropriate moment for scholars to apply their skills at critical inquiry upon themselves.

Also ripe for critical reassessment is the 1951–1953 period during which Dr. Mohammad Mossadegh served as Prime Minister. Virtually no research on that period has been possible until now. Given that certain of the key figures are still living, a rare period of immense scholarly potential is at hand. This new assessment seems especially appropriate because the essays here reflect a not uncommon view, often propounded by the Shah, that Mossadegh pursued essentially negative policies. Once he had nationalized Iran's oil, he had spent his only card. Rather than facilitating progressive social change, he impeded it. As Roger M. Savory puts the case in his "Social Development in Iran During the Pahlavi Era," Mossadegh "possessed the power to effect a social revolution but seemed devoid of ideas. . ." (p. 102).

Mossadegh's significance as a symbol of Iranian nationalism is ignored in this volume, but is clearly elsewhere appreciated. We can hope that a critical analysis of his government will be forthcoming which will not match the excesses of "Pahlavi historiography."

Yet another problem with these essays is their failure to use Persian language sources. Peter Chelkowski's essay on "The Literary Genres in Modern Iran" is a careful survey of much modern Persian literature. But with that exception there is scant reference to indigenous scholarship, much less Persian language materials. The scholarship of the post-Pahlavi era will clearly need to rely far more heavily on materials in the Persian language.

While these essays do not address the most salient contemporary issues, certain of them contain fascinating insights or oversights. Pio Filippani-Ronconi in "The Tradition of Sacred Kingship in Iran" argues that in Zoroastrian doctrines, "Kingship and Priesthood are considered to be the two fundamental poles of human society" (p. 56). The thesis-antithesis relation of the Shah and Ayatollah Khomeini during this year of struggle seems a contemporary manifestation of ancient doctrine.

One might wonder how in all these pages, including a specific essay by D. R. Denman, "Land Reforms of Shah and People," the name of Hassan Arsanjani can be missing. The former Minister of Agriculture was the key actor in the initial stages of the land reform in the early 1960s, when land reform was at its most decisive. Yet he is not remembered here.

Three sets of problems which confront any new government in Iran—certainly not peculiar to the Pahlavis—emerge in these pages. It is clear from these essays that once the land reform began, the government felt it had the right continually to alter patterns of rural ownership. The result has been disarray in rural society and agriculture and increased migration to the cities. Despite the turmoil—and the disaster of Iran's attempts to create massive agro-businesses which is ignored in this volume—agricultural production has been annually increasing at roughly 3.2 percent.

But with the burst in oil revenues, urban demand for foodstuffs increased to the point where in 1974/1975, Iran was spending $1.5 billion to import food—more than its entire oil revenues had been in 1970/1971. Iran's agricultural and food systems cry out for attention.

A second long-range problem is noted by Savory who observes that "social opposition to the concept of limiting family size has been tremendous" (p. 117) while Issawi projects a total population for Iran of 60 million by the end of this century. But Issawi repeats the misleading notion that "even with 60 million inhabitants, Iran would have a lower density of population than most countries" (p. 139). This is undoubtedly true if the calculation is made on the basis of Iran's total land area, but given the small percentage of arable land, such a large population would be a great economic burden.

A final issue which any new government in Iran must confront is the role of foreigners. In his excellent essay on "Iran's Foreign Policy," William E. Griffith specifies the "intimate" relation between Iran's domestic and foreign policies. The Qajars fell and Reza Shah came to power, he suggests, at least partly, "because of the humiliating penetration of the Iranian polity by foreign powers" (p. 369). Other essays make clear Reza Shah's refusal to accept foreign loans or allow sizeable numbers of experts from major foreign powers to participate in Iran's development.

His son took a different route. The extent to which the Shah's downfall was brought about by his close association with the United States, with 50,000 American experts in Iran, and with the "Americanization" of the Iranian military, is yet to be analyzed.

MARVIN ZONIS

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The Emergence of the Australian Party System. Edited by P. Loveday, A. W. Martin, and R. S. Parker. (Melbourne: Hale and Iremonger, 1977. Pp. xviii + 358. $19.95, cloth; $9.95, paper.)

Our understanding of the evolution of modern party systems has improved considerably in the last two decades with the appearance of a number of insightful works: Lipset and Rokkan, *Party Systems and Voter Alignments;* La Palombara and Weiner, *Political Parties and Political Development;* and Duverger's *Party Politics and Pressure Groups,* among others.

Now comes *The Emergence of the Australian Party System,* and it is, regrettably, as though the earlier works had never appeared. Loveday and his colleagues have produced a lengthy, well-researched study of the development of Australian state and federal parties between the 1880s and 1910s, but the study adds little to our understanding of the evolution of parties, either in Australia or more generally.

The major problem with the book is that the authors reject any interpretation in terms of general themes. The avoidance of interpretation is so consistent across the book's chapters that one almost has a sense of conspiratorial determination among the authors. Thus, in the chapter on Queensland, Joyce comments, "No single cause seems adequate to explain the changes in the Queensland political system between 1890 and 1910" (p. 168); and in the discussion of party evolution in New South Wales, Loveday, Martin, and Weller suggest that "it would be a mistake to search for overarching explanations of a socio-economic kind" (p. 244); and, once more, Jaensch argues that "No single factor provides an adequate explanation for the development of the South Australian party system. . ." (p. 296). (The rejection of interpretation is probably facilitated by this recurrent identification of interpretation with single-cause explanations.)

This problem applies even to the best of the book's chapters, a concluding chapter by Loveday on "Emergence: Realignment and Consolidation." Loveday admits to being "tempted" toward general explanations by the many similarities between party system evolution in Australia and elsewhere, but he concludes that Australia is unique because "the problem in the Australian context is to explain the transformation of non-party to party politics, not the conversion of middle class to mass parties" (p. 460). This argument can be faulted on two counts. First, part of the problem *is* to explain the evolution of mass parties for, as is illustrated in almost all of the book's chapters, one aspect of Australian party evolution was the development of mass, class-based labor parties. Second, Australia is hardly unique in having experienced a transformation from non-party to party politics in this period. Many countries, including such a seemingly non-comparable country as France, experienced similar transformations during much the same period, often concomitantly with the development of mass parties.

Nonetheless, this concluding chapter remains interesting, and in large part because Loveday briefly forgets his antagonism toward explanation and offers an interesting analysis of party evolution in Australia (pp. 480–87). The analysis draws on several theories, and makes this the one chapter worth reading by students of party development.

The interpretive failings of the rest of the book are especially unfortunate because the book reflects extensive and painstaking research. That research has produced impressive detail on the chronologies of party and legislative behavior during the early years of Australian parties. Other students of Australian parties may find this detail useful in developing the explanations which these authors have for the most part disdained.

JOHN CLAYTON THOMAS

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. . . **and the Desert Shall Rejoice: Conflict, Growth and Justice in Arid Environments.** By Arthur Maass and Raymond L. Anderson. (Cambridge, Mass.: MIT Press, 1978. Pp. 447. $19.95.)

Irrigation institutions, like most political institutions, exist for the purpose of resource allocation, conflict resolution, and to defend that ephemeral quality—justice. If they are to be successful at all, institutions must be adapted to the technological conditions within which they must function. These conditions—principally the flow nature of the resource and its unpredictability—have dictated distinctive norms of behavior and attitudes. It is to the distinctive character of these institutions that this book is addressed, using for its raw material case studies of irrigation practices in six communities: three in Spain and three in the United States: northern Colorado, central Utah, and the Kings River Service area in California.

As the authors put it, this is a "parochial communities" view of institutions as distinguished from the view usually expressed by students of political development and modernization. The latter tend to focus on central institutions and their capacity to encompass traditional or parochial communities within their control. This "bottom-up" view reveals that rather than being brought within the control of centralizers of the modern nation-state, irrigation communities have been remarkably resistant and independent. The authors' central conclusion is that Wittfogel was simply wrong: irrigation agriculture does not tend toward centralization of political power, in part because no government is so despotic as to shut off the tap once the supply and its delivery system have been provided, and in part because the irrigators themselves have contrived institutions that reflect their consensus about water allocations and the procedures by which those allocations should be made.

The major portion of the book—355 of its 400 pages—are devoted to a very detailed accounting of the development of irrigation practices in these six locations. Nearly one-third of the book alone is dedicated to the Kings River service area. In each case there is a discussion of the physical conditions, historical development, the governing institutions, the procedures and rules for allocating water both in times of abundance and shortage, and finally a discussion of community objectives in terms of the norms of equality, equity, efficiency, conflict resolution and popular participation (although this systematic discussion of community objectives is strangely lacking from the Kings River case).

There are significant differences in the practices of these irrigation communities in terms of standards for allocating water, the methods of organizing and decision making, and procedures for adjudicating disputes when they arise. But all reflect a community consensus about what is fair when shortages occur. Moreover, all reflect an awareness of the need for consensus and effective operating procedures in order to retain the capacity to deal effectively with central institutions. As the need for water expanded, and it was realized that such needs could be fulfilled only by the higher levels of government, the local institutions enhanced their bargaining power by the strength that they displayed.

The central thesis of the authors is especially relevant to U.S. students of water policy as the Carter administration endeavors to impose new standards on water development projects and to adopt water conservation as a major program to ensure adequacy of water supplies. A reasonable lesson derived from this book is that irrigation farmers may be induced to adopt water conservation measures if the federals will simply pay for it; if the federals try to impose constraints on irrigationist behavior, as EPA did in the application of the NPDES system, they simply will not work because of the massive resistance of irrigation interests.

This point leads to an observation that curiously escapes the authors of the book: non-irrigation interests in the United States are seldom hostile to irrigation agriculture. Despite the intellectual assaults of economists and the competition for scarce funds from urban interests, irrigation agriculture, at least in the United States, has done very well politically. Perhaps this reflects our agrarian heritage, perhaps the magic associated with mastering nature, especially through making the desert blossom. In the western states themselves, where competition for water is keenest, few want water to be taken from irrigation agriculture. Far from being hostile and centralizing, federal institutions as typified by the Bureau of Reclamation, are decentralizing, accommodating and submissive to the local water interests.

On the other hand, federal interests *are* bureaucratic and determined to protect themselves. It is one thing to demonstrate, as the authors do, that central institutions cannot compel behavior of irrigationists once the projects are built, but it is another to demonstrate that these bureaucratic agencies may not wield substantial influence when the local interests want something federals are loathe to give. Having adopted operational criteria, they are likely to defend them despite reasonable argument or potential injury. And in crisis situations, as in the western drought, the bureau was quite capable of reducing agricultural deliveries in California by 75 percent, while reducing municipal and industrial deliveries by 50 percent.

In the concluding chapter, the authors consider the meaning of irrigation agricultural practices for economic growth, equity and equality. Having utilized simulations of the procedures used in each community to estimate the benefits of each approach, they then compare the various procedures in terms of their economic efficiency. They conclude that a demand plan procedure and a market structure for buying and selling water rights are not only most efficient but also provide greatest equality as measured by several standards such as the percentage loss in income per acre. Such procedures and structures are more often than not rejected, however, because of fear of instability and loss of local control. The authors also observe that land speculation, whatever the distribution of profits and losses in the development process, actually was more likely to contribute to stable communities of smaller farms than was cooperative development.

Despite these observations, this is a curiously non-economic book. If one thinks of the charges of corporate interests gaining dominance in the Central Valley of California and the existence of districts in which a few large interests predominate, one finds little discussion of such allegations and/or facts. Perhaps if the California example had been the Westlands Water District, the picture might have been somewhat different. The authors do observe, however, that irrigation farmers are prepared to accept rules that guarantee higher levels of inequality because the rules promote stability and local control and because farmers may even subscribe to the ethic of hard work as the basis for economic success.

In sum, this is an important book. The research effort was prodigious, the exposition lucid (albeit so detailed that the reader has difficulty in digesting it all), and the results significant. The authors themselves raise the question as to whether other local institutions are equal in their capacity to resist external influence and to maintain their integrity. Certainly that question should be addressed by others in other contexts.

DEAN E. MANN

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Power and Class in Africa: An Introduction to Change and Conflict in African Politics. By Irving Leonard Markovitz. (Englewood Cliffs, N.J.: Prentice-Hall, 1977. Pp. xii + 398. $12.95, cloth; $8.95, paper.)

Irving Leonard Markovitz's *Power and Class in Africa* is a work of impressive scholarship. Encyclopedic in the wealth of material it presents, this book will be consulted as a useful source of reference by both specialists and non-specialists. Put simply, Markovitz has attempted to synthesize and review all of the significant literature on the political economies of independent African states. That he has succeeded is alone worth the price of this book.

Power and Class in Africa, however, is fundamentally a textbook, and it is on this ground that it must ultimately be judged. But by what standard? Any attempt to arrive at a single set of criteria by which to evaluate and choose a text is as much a reflection of what questions a given instructor regards as important for students to understand as it is an evaluation of the author's analytical skills. At the risk of being somewhat presumptuous, I list my criteria as basically four: (1) The text should explore the *reciprocal* relationships between the basic political and economic factors that constitute the structure and dynamics of contemporary African societies. (2) The text should present a historical (though not necessarily a dialectical) analysis of the emergence and evolution of these factors from the precolonial period, through colonialism, and to the present. (3) The text should consider the alternative ideological and policy options available to political leaders of independent African states, their impact, and their attendant costs. (4) The book should be exciting for undergraduates to read.

Against these criteria, particularly the first and the second, *Power and Class in Africa* is more rewarding than virtually all of the few alternative texts now on the market. Markovitz is on particularly solid ground when discussing the process of class formation, and its significance for the distribution of political power. This is the central theme of his book, and it is not surprising that he develops it well. His discussion of the historical developments which led to the emergence of the class structures of contemporary African states, particularly in the late nineteenth and early twentieth centuries, is especially detailed and rich in basic information.

Power and Class in Africa, however, does not fare as well in respect to the third and fourth criteria. In my opinion, a delineation and explanation of the distribution of power is only half the story. One must also ask the question, "power for what?" Although Markovitz states on several occasions that he is concerned with the basic questions of who gets what, when and how, and although he devotes the final third of his book to a discussion of how those with power have consolidated their positions, the reader does not come away with an appreciation of the conflicting political forces which shape the policies of contemporary African states. It is one thing, for example, to discuss the creation and continuity of an "organizational bourgeoisie," as Markovitz has done superbly in chapter 10. It is quite another to discuss the internal conflicts within this class, and especially its relationships, both harmonious and competitive vis-à-vis other centers of power, i.e., the nascent national bourgeoisie, the rural power elite, the peasantry, ethnic blocs, multinational corporations and foreign powers. Although Markovitz considers the development of several of these on a one-by-one basis in exhaustive detail, his discussion of the relationships between them, especially the substance of the competing policies they demand, is limited, with the result that he presents an incomplete description of the stuff of which African politics is made.

It is not enough, for example, to lament, as Markovitz periodically does, about the extent of inequality on the continent. He must also consider what the likely impact, tradeoffs, and reaction will be to policies designed to ameliorate this basic problem. The leadership of a country, and I will take Tanzania as a case in point, may enact a progressive income tax and other restrictions to reduce the amount of wealth accruing to the "organizational bourgeoisie" relative to the rest of society, with the unexpected result that many members of this class become corrupt and neglect their official duties. Or, a government might pursue a policy of paying low prices for agricultural produce, as the Tanzanian government also did prior to 1975, to reduce the cost of food to urban workers and the amount of income flowing to rich farmers. In this case production fell, and the smuggling of produce out of the country became rampant.

Markovitz's insufficient consideration of the policy alternatives available to African states mars an otherwise sound presentation. This omission, however, when combined with the book's exhaustive detail, results in a volume that is unlikely to excite the average undergraduate. The greatest strength of *Power and Class in Africa,* its richness, is also its primary weakness as a text. Most undergraduates will be unable to digest the huge quantity of data Markovitz expects them to consume. Superb scholarship and a flair for presenting basic material in a crisp yet comprehensive manner do not always go hand in hand. Such a balance will have to await an effort by another, perhaps less ambitious scholar than Markovitz. Until then, *Power and Class in Africa* will partially fill a serious gap in the literature

JOEL D. BARKAN

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Classe, religion et comportement politique. By Guy Michelat and Michel Simon. (Paris: Presses de la Fondation nationale des sciences politiques, 1977. Pp. xii + 498. FF110, paper.)

This is a very fine intellectual construction, complex in design, subtle in detail, and of such methodological scrupulousness that the reader is often required to consider the authors' own question—is our procedure at this stage defensible? What the design calls for is nothing less than a structure resting on two pillars, qualitative and quantitative. The former is built up from 77 interviews originally conceived as part of a research program, mounted in 1966, into perceptions of the political field, especially the structuring of political attitudes. The interviews were non-directive in the hope that the inquiry, not the inquirers, would give definition to the field. Guy Michelat and Michel Simon then set out to verify what the interviews revealed. This, the quantitative part, takes up most of the book: it is so rigorous that it gives the general impression that Descartes might have written it had he done graduate work under Lazarsfeld. That is not to say that it is unreadable, only that it is demanding. Fortunately, the authors proceed step by step, with frequent recapitulations, so that anyone can follow who has patience and a sense of adventure (as well of course as a reading knowledge of French).

The qualitative research revealed two different structurings of the political field and more broadly two visions of the world characteristic of two groups of individuals typed by the authors as avowed Catholics and irreligious communists. To these two groups belonged different patterns of attitudes, norms and values, one centered on the Catholic religion, the family, and so forth, the other on feelings of class identification. That sets the scene for a quantitative assessment of the reciprocal relationships between class and religion as tending, although only in a probabilistic sense, to govern political behavior. Other variables get some attention (sex, type of habitat, level of education, etc.), but are not deemed central. Political behavior always means voting behavior, which is always verbally manifested (i.e., intentions, elicited by questionnaire), but the authors believe that the deeds match the words (p. 150).

Class is resolved into its objective and subjective components, the former given what may be called a quasi-occupational meaning ("quasi" because *socio-professionnelle* is not identical with social class, only—it would seem—the least inadequate indicator of it, p. 139, n. 7). Objective class is found to influence the vote via subjective class, but continues to exert its own independent influence, the final result being cumulative. Thus the probability of voting left, especially communist, was at its maximum (60 percent) among workers who were the children of workers and who perceived themselves as working-class. (However, 26 percent would vote gaullist or centrist, prompting the authors to remind us that the interaction takes place within a sociohistorical context in which people are exposed to signals from many sources, p. 257.)

And so to religion—meaning Catholicism, since the number of "other confessions" caught in the research net was too small to keep and make use of. The authors first reexamine the venerable ecological relationship between the high proportion of conservative votes in regions of regular religious practice, the Left being strong where such practice is weak. From a reanalysis of public opinion polls covering the first round of voting in the presidential elections of 1965 and 1974, and the legislative elections of 1966 and 1973, Michelat and Simon conclude that the relationship still holds. However, some interesting tendencies were observed, e.g., in the legislative elections, among those without religion, support for the Communist party fell but support for the non-communist Left and the Center rose. In the cantonal elections of 1976, the socialist Left may have gained ground even among the most faithful churchgoers (p. 376, n. 52).

The authors' own findings are congruent with the traditional view, but they proceed to acknowledge that the ecological inverse does not hold: there are departments of weak religious practice that exhibit conservative tendencies. Turning a gun on themselves, they even speculate whether the apparently verified proposition is not susceptible of another interpretation—that it is not Catholicism as such but *a* specific Catholicism that happens, for historical reasons, to be associated with political conservatism, so that what is at work is a contextual variable operating in certain regions (p. 407). Their final judgment, however, is that the expected relationship does hold (pp. 413 and 446).

Finally, religion and class, objective and subjective, having been put asunder, are conjoined the better to explain political behavior. One finding among many is that the effects are cumulative, e.g., it is among workers objectively and subjectively who have "lapsed" or are without religion that the left vote (communist and non-communist) is at its maximum (73 percent), and the right vote (gaullist and centrist) at its lowest (16 percent). Here again, it is claimed, the findings are congruent with the expectations derived from the qualitative part of the research. Thus it is the action and interaction of the three variables that accounts, probabilistically, for voting behavior and possibly other forms of political behavior. Can one say which variable contributes most? For what it is worth, a tree analysis points to religion, followed by subjective social class, with objective social class contributing little.

On that (for me) rather dubious note, the book ends. All in all, a very fine piece of work, perhaps more to be admired than enjoyed, but certainly to be read by specialists in French politics but also by political scientists in a broader sense and by the sociologists of politics.

GRAHAM WOOTTON

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West Bank/East Bank: The Palestinians in Jordan, 1949–1967. By Shaul Mishal. (New Haven, Conn.: Yale University Press, 1978. Pp. xiii + 129. $9.50.)

Shaul Mishal, of Tel-Aviv University, uses Hebrew-language publications as well as material from Jordanian archives captured by the Israelis in 1967. Unfortunately, *West Bank/East Bank* promises more than it delivers and is essentially a long essay with a definite and limited objective. It is not, as the subtitle claims, about Palestinians in Jordan. It is not entirely about Palestinians on the West Bank since it ignores those Palestinians in refugee camps, for instance. Neither does it cover fully or adequately the political (let alone the social, economic or cultural) dynamics of Palestinian life on the West Bank. According to Mishal, "Since 1967 the Palestinian issue has been a major and complex element pervading world politics." Mishal's objective, therefore, is "to achieve a sense of balance" in order for us not to be overwhelmed by this resurgence of Palestinian identity. In other words, instead of a Palestinian entity or state on the West Bank (and Gaza), he wants to explore the idea of continuing foreign rule over the West Bank the way Jordan ruled from 1949–1967.

With this objective firmly in mind, Mishal proceeds to explore the relationship between the Hashemite regime and the Palestinian political and ideological leadership. As a result, while much that is presented is factual, the end result is rather inaccurate and misleading. In part, this is because the author is selective in his sources. Information from the Jordanian Security Services archives adds little of value and often merely corroborates material from other available sources. Also, the Hebrew-language sources are relatively few in number and, in any case, are mostly secondary sources, chiefly ones providing Israeli interpretations of attitudes and behavior of Palestinians on the West Bank. In fact, almost one-fourth to one-third of all such citations come from one source whose English title is "The Political Parties in the West Bank Under the Hashemite Regime," edited by Amnon Cohen and mimeographed in 1972.

In Mishal's account the Palestinians never emerge as a living, thriving community. One cannot help feeling that this is intentional. Hence the use of "Palestinian primordialism" to refer to Palestinian nationalist aspirations, for instance. Also, Mishal claims that the Palestinians on the West Bank, having been prevented from realizing their desires, i.e., Palestinian nationalism and statehood, adopted what the author terms "floating identity." Thus they allegedly had Arab, Muslim, Jordanian and Palestinian identities simultaneously and tended to display at any one time the identity that caused them least harm or brought them most good. Somehow this is presented as if it were different from contextual or situational identification. In any case, to "prove" his point, the author presents the views of political parties on the West Bank such as Muslim Brotherhood and the Ba'th and how they worked for ultimate political salvation through a Muslim or an Arab state. What this has to do with "floating identity" is not clear, since it surely was not the case that Palestinian followers of the Muslim Brotherhood were also and at the same time Arab Nationalists, Jordanian nationalists *and* Palestinian primordialists (i.e., nationalists).

Two obvious mistakes should have been caught by the readers of the manuscript. On p. 105, last paragraph, two references to the "party system" should read "election system." More disturbing is the statement: "The Baghdad Pact, one of the first manifestations of an integrative concept of Arab unity, came into being in late February 1955, when Iraq signed a military agreement with Turkey and Pakistan" (pp. 53–54). Needless to say, Iraq was the only Arab country to join the pact which was generally viewed as a measure to *combat* Arab nationalism and unity.

In sum, the book suffers from the fact that Mishal begins with a thesis which he sets out to "prove" and, in the process, chooses his sources selectively, arranges the data in a manner suitable for his purpose, and resorts to expressions such as "Palestinian primordialism" and "floating identity." How inadequate this procedure is may be better illustrated through a hypothetical parallel history of Zionism written in this vein. Such a history would discuss "Jewish primordialism" and refer to a "floating identity" as illustrated by the simultaneous Jewish attachment to Russia (or Poland, France, the U.S., etc.), pan-Jewish movements, secular Zionist organizations, and communist or socialist programs. Of course, such a history would also teach us (or should have taught us) how to avoid establishing a Jewish state!

MICHAEL W. SULEIMAN

Kansas State University

The Politics of Industrial Relations: The Origins, Life and Death of the 1971 Industrial Relations Act. By Michael Moran. (Atlantic Highlands: Humanities Press, 1978. Pp. 195. $16.00.)

Michael Moran's excellent study addresses a key problem plaguing the British polity: how to reconcile the new system of functional representation with the traditional parliamentary system of government. His work addresses this theoretical issue by exploring the development and ultimate failure of the notorious Industrial Relations Act of 1971.

The Industrial Relations Act was the Conservative party's strategic response to its governments' successive battles with trade unionism, as well as to that moment's chaos in industrial relations. Conservatives sought to bring the union movement within a framework of laws for the first time. Their specific purpose was to restore discipline in Britain's strike-ravaged industrial relations and thereby reduce the disruptive influence of unionism on economic policy. Such intransigence in several forms had paralyzed the policies of both Labour and Conservative governments since the end of the Second World War.

In this book, Moran explores the sorry postwar relationship between the unions, the Conservatives, and their several governments. He describes the evolution of Conservative thinking on the reform of industrial relations and examines the struggle between the Trades Union Congresses, as spokesgroup for trade unionism, and the Heath Government over the passage and then the implementation of the Industrial Relations Act.

Moran concludes, rightly, in my opinion, that the troubles suffered by the Industrial Relations Act express the wider problem in the system of government itself. That problem is the challenge of the functional system of representation to the existing parliamentary system. The defeat of the Industrial Relations Act demonstrated this threat. The act failed because the functional representatives—both the unions and the employers—refused to accept it.

Moran goes on to insist that some means of accommodation between the two systems must be found. The Wilson-Callaghan Labour Government explicitly recognized this in its conduct of the social contract which brought the union movement into the closest consultation at the very center of decision making. While that arrangement has been useful, it is hardly a permanent answer nor one which would be as available to the Conservatives. Moran explores several other schemes including the often-suggested reform of making the House of Lords into a functional body—which he likes best—but in the end admits that he has found no perfect solution.

Michael Moran's book is first-rate. He has produced a thoroughly researched and thoughtful piece of work. His writing style is interesting and the book is well organized. Most importantly, I find his analysis of the problem at the root of the struggle over the Industrial Relations Act quite convincing. Too bad he fails to provide a more imaginative "way out" for Britain, but no one else has produced that bit of magic either.

GERALD A. DORFMAN

Iowa State University

Les Guerres Grises: Résistance et Révoltes en Angola (1845–1941). By René Pélissier. (Orgeval, France: Pélissier, 1977. Pp. 630.)

La Colonie du Minotaure: Nationalismes et Révoltes en Angola (1926–1961). By René Pélissier. (Orgeval, France: Pélissier, 1978. Pp. 727. FF 280 for both volumes.)

These companion volumes represent the culmination of a decade of research on Angola by a French scholar. In original form the material herein constituted a doctoral dissertation; in the present volumes it represents a political history of Angola, richly documented and illustrated with maps, charts, and tables. Although Pélissier was denied access to archives in Lisbon prior to 1974, he has relied on a synthesis of secondary sources. He focuses on the unceasing African resistance to Portuguese rule in Angola. Whereas Portuguese officials and other observers have described aspects of this resistance, in no single work has a broad sweep of this history yet appeared in such detail. Thus these volumes guide the reader toward the task of reconstructing an African history of Angola during the Portuguese colonial period.

In the first of these volumes Pélissier sets forth the proposition that the struggle for Angola essentially dates from the middle of the nineteenth century; evidence that the Portuguese never fully controlled the colony is drawn from the more than 180 military campaigns they launched between 1848 and 1926. Pélissier describes the different phases of this period of conquest. Nineteen chapters look closely at events and developments in each of the principal regions of Angola. African uprisings, Portuguese military operations, and warfare are reviewed in detail. Pélissier shows that between 1879 and 1926 the Portuguese were at war nearly half the time and were forced to commit more than 30,000 troops to their cause. In addition to tracing these battles, Pélissier delineates the causes of the resistance and revolts by examining the impact of colonial administration, the commercial penetration, as well as taxation and slavery. The exploitation of Portuguese civilians and military is exposed.

The second of these volumes covers the period from the end of the conquest period in 1926 to the outbreak of African warfare in 1961. A first part describes the social and political conditions as of 1961, covering population data, the administrative system, nationalism, religious forces, and white separatists forces. Ethnic groups are related to the emerging national liberation movements of the time. A second part of the book deals with three revolts in 1961, including the urban uprising in Luanda during February, the revolt in Baixa de Cassange during February and March, and the insurgency in northwestern Angola in March. Considerable attention is devoted to the last of these events, in particular to describing the rise and decline of the Bakongo resistance and war. The successes and failures of that momentous year marked the beginnings of an exhaustive war of liberation culminating in independence many years later.

These volumes are invaluable for scholars who wish to delve into the African struggle for Angola. They serve as a framework for future study and research and should prove especially useful to political scientists interested in southern Africa.

RONALD H. CHILCOTE

University of California, Riverside

Ireland at the Polls: The Dail Elections of 1977. Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute, 1978. Pp. 199. $4.75, paper.)

This collection of essays by several of Ireland's most noted political scientists serves a number of useful purposes. For the general reader it provides a highly readable introduction to electoral politics in Ireland, and the first authoritative account of the background, conduct, and results of an Irish general election. For students of Irish politics there is an additional bonus: the record of the highly unusual, if not "critical" election of 1977.

Near the top of the list of things noteworthy about this election were its many surprises: "an unexpected change of Government, the highest vote for a single party and biggest parliamentary majority in the history of the state, the precipitate resignation of two national party leaders, the defeat at the polls of three cabinet ministers, and the embarrassment of virtually the entire corps of observers and commentators, academic and journalistic, all of whom had failed to predict the outcome" (p. 133).

Lurking behind this intriguing tale of the unexpected seems to be another message: the coming of age of Irish elections by way of more modern content and technique, and the almost wilful failure of the "inside dopesters" to comprehend the meaning of these changes. Elections in Ireland have traditionally been characterized by their combination of ideological rhetoric and highly personalized and parochial concerns. The days and heroes of the Irish "Troubles" enjoyed an afterlife celebrated seasonally at campaign times, a celebration which added to, but never disguised the deeply idiosyncratic flavor of local constituency contests.

These "traditional" features of Irish elections have been slowly receding, a transition spotlighted by the events of 1977. The Ulster issue was pushed to the sidelines. The dominant items on the voters' minds—as measured by the polls—were not parochial concerns but economic issues such as unemployment and inflation. Techniques were changing as well. Tried-and-true methods of Irish electioneering, such as the gerrymander (in this election renamed the Tullymander after the minister whose artwork shaped the new constituency boundaries) failed those in power. By contrast, the opposition party (Fianna Fail) sported several technical departures; "the early planning and sophisticated professionalism of the promotion campaign; the identification of crucial issues and segments of the electorate; and the use of market research to monitor and correct the campaign" (p. 119). Perhaps one of the best symbols of the changing times was the shift from the wall poster to the tee-shirt as a campaign gimmick.

To suggest that Fianna Fail's victory was that of a more modern campaign over a traditional one would be, perhaps, an overstatement. But the bast proportions of their victory, which produced the biggest turnover of deputies at a single election since 1927, suggests that this was much more than the perennial desire of electorates to "turn the rascals out." How much more, and why all this was happening in 1977 does not really emerge from these essays.

The Irish, frequently for good reason, have been skeptical of "scientific" studies of their own highly insular politics. But this valuable collection of essays, several of which are in the "scientific" mode, tells a tale which is likely to erode such skepticism in the future.

PAUL M. SACKS

University of California, Los Angeles

Civil War in China: The Political Struggle 1945–1949. By Suzanne Pepper. (Berkeley: University of California Press, 1978. Pp. xxi + 472. $18.50.)

One of the more enduring pastimes and occupations of Westerners during the momentous latter half of the twentieth century has been to observe and to interpret events in the non-Western world. The record of accuracy in observation or in interpretation is not an enviable one. No part of this assorted group of observers has been more devoted or assiduous, or had a more fascinating subject than those known as the China Watchers; and their record of failure compares favorably with that of those who have watched in other directions.

There are many reasons for this frustrating experience, but certainly one of the most important is that Westerners, and perhaps especially Americans who make up a large bulk of the profession, look at China through their own eyes and experience, and tend to make judgments in terms of their own values. Seldom do they attempt to evaluate how things look from the other side of the river. Understandable as this may be, the results at best are conducive to misunderstanding and sometimes a great deal of mischief.

Now that at long last civility is being restored to Sino-American relations, *Civil War in China: The Political Struggle 1945–1949* by Suzanne Pepper is a splendid contribution to our understanding of that part of the Chinese Revolution between the defeat of Japan and the proclamation of the Peoples' Republic of China which has affected history for all time. Writing largely from Chinese sources, many of them previously unused, Pepper gives a dramatic account of how Chiang Kai-shek and his Nationalist Government, which in the fall of 1945 seemed to have the Mandate of Heaven if anyone ever did, could in less than four years lose every shred of it to the Communists. It was not primarily a military story. It was a political one in which the corruption and incompetence of the Kuomintang in the end turned every section of the population against it.

Business people and industrialists had no illusions as to their future under the Communists; and the intellectuals had good reason to suspect that untrammelled freedom of expression and the continuation of their traditionally preferred position in Chinese society would not be acceptable. Nonetheless, both came to feel that anything would be preferable to the Kuomintang. Even these essentially urban defections would have been insufficient had the Communists been unable to mobilize peasant support. Here Pepper makes a major contribution. The landlord-tenant problem, ownership of land, was not a major problem in north China, and the Communists muffled it prior to 1949. What was needed was a whole new definition of political and social relationships, namely, of power, who exercises it, and to what ends. It was precisely here that the Communists demonstrated their finest understanding of the Chinese people. Having acquired this support, the Communists had become invincible, as those of us who lived there at the time did understand.

Apologists for the Kuomintang have tried to make a case that although it lost, the Communists did not win; they simply flowed into a power vacuum. The thesis never did make much sense, but Pepper shows that it simply was not true. Even before General Marshall gave up in disgust, no one in China, the general included, any longer believed anything the Kuomintang said, and everyone knew the Communists meant exactly what they said and said exactly what they meant. One might not like it, but the security this promised was preferred to the chaos and misery that had wracked China for 50 years. And if the military part of the story was not primary, still, the impeccable behavior of Communist troops was unprecedented in Chinese history, whereas that of Kuomintang troops was conventional in a society in which the soldier was traditionally dragooned from its dregs and behaved accordingly.

It is fashionable in some circles to attribute the victory of the Communists to the opportunity offered by the Japanese War to identify themselves with Chinese nationalism. Pepper does not ignore this argument but she does, I think, place it in perspective. The Communists did exploit it to the fullest, but they also used it to learn and correct mistakes made in Kiangsi. The Kuomintang had an even better opportunity and missed it on every count. If one wishes to speculate as to why Japan invaded when it did, a major reason must have been an estimate that Chinese nationalism had reached a point which would soon make it inviolable. It was Mao Tse-tung who also understood this, not Chiang Kai-shek. Chiang's obsession with the Communists and with Confucian feudal politics, which he understood only too well, had a kind of fatal self-fulfillment about it.

The Chinese being a very private people about their own internal affairs, we are not likely to have a better account of the politics of a major turning point in history than this one.

JOHN F. MELBY

University of Guelph

Social Democracy in Capitalist Society: Working-Class Politics in Britain and Sweden. By Richard Scase. (Totowa, N.J.: Rowman and Littlefield, 1977. Pp. 184. $15.00.)

Questions about ideology bedeviled political scientists before Marx, but his writing set the tone for research and theory on ideology in industrial societies. Without elaborating on the development of class consciousness, Marx makes it clear that ideology arises from objective conditions. Attempts to investigate whether differences in objective conditions do indeed give rise to differences in ideology and political activity have confronted two barriers. First, even if the hypothesis is correct, the result may not occur because the variation in conditions across advanced societies represents only a truncated portion of the range Marx envisioned. Second, given the infrequency with which ordinary citizens link issues into ideologies, it is virtually impossible to hold that meaningful class consciousness springs automatically from conditions in the real world.

Political sociologists have lately taken a more Weberian tack, arguing that causation runs the other way. That is, it is not so much the fact of inequality as the changing views which leaders of working-class movements and parties take toward it, which determine whether and how such issues come into the political arena. This makes for a more complex picture. Where politics was once epiphenomenal and parties little more than creatures of their class constituencies, politics and conflict now move toward center stage. Party leaders, though dependent on their class constituencies, play an important role in shaping the demands to which the government responds. Frank Parkin has articulated this approach most forcefully, proposing (in *Class Inequality and Political Order*) a classification of party ideologies in terms of their potential for fostering redistribution. A working-class party might adopt either a *subordinate* value system, which accommodates and legitimates existing inequalities, or a *radical* one, opposing extant distributions. Although the radical ideology may seem a "mere reflection" of class interests, its more important function is to translate latent objective interests into electoral demands and a political program. The party contributes to class consciousness by acting as exemplar of the class's political interests. In *Social Democracy in Capitalist Society,* Richard Scase, a British political sociologist, attempts to test Parkin's thesis in a comparative context, focusing on the Labor Party in Britain and the Social Democrats in Sweden.

Although an hypothesis about the effects of party leadership on the political consciousness of citizens could be investigated solely with attitudinal data, Scase begins with the prior question of policy outcomes. Stipulating (largely on the basis of campaign announcements) that the Swedish party is the more radical, Scase expects that the Social Democrats will have achieved a greater equalization of income and wealth than the British Labour party. Relying on a wide range of published research and government statistics, he finds precious little evidence for the belief that redistributive policies have equalized economic rewards or enhanced the opportunities for upward mobility by members of the working class in Sweden. This finding—that in objective terms some four decades of Social Democratic rule have left the Swedish working class in virtually the same position as Labour's episodic participation in government has left the British working class—suggests that party ideology may not matter in the final analysis, i.e., in the ability of the party to equalize the ownership of social wealth. It seems to imply that, if the parties have different effects on their constituents' political beliefs, then these beliefs can be nothing other than false consciousness. These findings may be explained by a closer look at the policy-making process. It is unfortunate, then, that Scase does not address current literature on policy making in the two countries, particularly Heclo's argument (in *Modern Social Politics*) that party competition has not mattered greatly to the development of the welfare state in either nation. While this growing body of literature would not necessarily have embarrassed Scase's theory, confronting it might have honed a sharper set of testable propositions.

As for the effect of party ideology on citizen beliefs, Scase's choice of these two countries makes the demonstration of his hypothesis difficult, since their leftist parties are so similar. The measured effects might have been different had one of these parties been contrasted with the American Democrats or Italian Communists. Data come from a small-scale survey of workers in two factories. They reflect the laudable subtlety of his instruments, and their story is complex and sometimes contradictory. Scase reads the findings as supporting his expectation that Swedish workers will manifest a clearer appreciation of social inequalities and be more resentful of their continuation than the British. I find this interpretation tendentious. The image that emerges shows British workers convinced that status is transmitted by the family as a locus of advantage within an essentially unequal society, and Swedish workers perceiving a meritocratic system in which equality of opportunity operates through education (cf. pp. 102, 108). Scase concludes that Swedish workers are more "radical" because of their greater belief in equality, but this interpretation conflates a belief in equality of *opportunity* with the traditional socialist conception of equality, with its emphasis on *outcome*.

There are other annoying theoretical quirks. For instance, in a book focused on the question of change in working-class conceptions of social structure and political power, the notion of a political generation is absent—despite its centrality to the prevailing (Butler and Stokes) explanation of political change in Britain. Absent also is any consideration of the growing American literature on belief systems and on the interplay of political leadership and issue-conceptualization by the mass public. It is incredible that Scase is ignorant of this body of work—convergent with that of Parkin and others—yet neither his conceptualization of the problem nor his citation of the literature suggests that he attempted to integrate this research.

Despite its flaws, however, this ambitious work makes an important empirical contribution to the neo-Weberian theories of class structure currently emerging through the efforts of Parkin, Anthony Giddens and others. If it provides a model for further, better work, it will have fulfilled an important niche in the literature.

M. STEPHEN WEATHERFORD

University of California, Santa Barbara

Soviet Perceptions of the United States. By Morton Schwartz. (Berkeley: University of California Press, 1978. Pp. viii + 216. $12.50.)

The Soviet perceptions considered in this volume are those of the members of Moscow's Institute for the Study of the U.S.A. and Canada, founded in 1967. The book's primary data base is the institute's monthly journal *USA: Economics, Politics, Ideology*, and, secondarily, a number of volumes either sponsored by the institute or written by persons affiliated with it.

Initially, there is a brief consideration of attitudes toward American politics, society and the economy. The author reports that the institute's analysts have considerable respect for the strength and resiliency of the latter two because, despite the social and economic problems they have examined so closely, they never expected a social or economic collapse. As for politics, they have made considerable progress in moving beyond narrowly orthodox, simplistic approaches toward a more sophisticated understanding of the political system's complexities.

The volume is chiefly concerned with how the analysts treat the making and conduct of foreign policy. They have abandoned the earlier orthodox penchant for considering American foreign policy to be insatiably imperialistic because of its origins within the arcanae of the innately expansionistic economic elite which totally dominated the political system. They now see policy as a less venturesome and combative, more moderate and realistic product of a clash among opposing political forces and constituencies with the more "sober-minded" among them gaining ascendency over the past decade or so. The analysts attribute this to a growing realization in America that the correlation of forces, or balance of power, in the world has been shifting against the United States and toward socialism, that is, toward the forces headed by the Soviet Union. Consequently, in recent years U.S. policy has been less characterized by force, or even by the threat to use it, than heretofore.

The analysts have not seen this tendency as undeviating. They have been troubled perenially by what they term zig-zags in American policy. And following their excited optimism produced by the numerous summit agreements in the Nixon era, in recent years their enthusiasm has been tempered by the tougher policies they have noted in subsequent administrations.

A major motif of the book is policy advocacy by the institute as well by Schwartz himself. He sees the institute as a major supporter of detente and overall moderation in Soviet foreign policy. He feels that the United States ought to act moderately in order to encourage this tendency which, as he notes, leads a precarious existence in the Soviet Union. At the same time he advocates that a strong American military posture be maintained, "which, somewhat paradoxically, is vital to the argument of Soviet moderates. . ." (p. 169).

If this is not the first American book to deal with the broad topic indicated by the title, it is the initial detailed treatment of the thinking characteristic of this important institute which is indubitably influential in shaping Soviet foreign policy. The book's most apparent weakness is that its unusually heavy use of extensive quotations coupled with minimum analysis, interpretation and contextual explanation are more suggestive of a report than a study, and they impart a laconic and abrupt quality to the book.

A book limiting itself to such a narrow purview could be expected to have clarified to a greater extent than is done here the specific purposes and functions of the institute as those become evident in the journal. For example, the overwhelming majority of the articles in the journal concentrates on using the very latest American books, journals, news magazines and newspapers in order to report contending American views on a variety of topics in an unusually even-handed way, for a Soviet publication. This allows the journal to document changing American perceptions, moods, attitudes and institutional relationships and to comment upon the immediate significance of all these things.

Important as that is as an exercise in reporting on the current situation, it is not necessarily productive of changes in long-term Soviet perspectives on the American political system which are strongly influenced by general theoretical (ideological) considerations. The latter are particularly salient in major policy statements made by the top political leaders. The significance of this important disparity ought to have been probed in a book involving policy advocacy.

The book is clearly useful within its substantial self-imposed limits. Schwartz could have made it more valuable by analyzing the materials more rather than by so literally reflecting them and by considering some of the broader questions raised by the institute's activities and position in Soviet academic and political life.

These criticisms aside, Schwartz has given his readers a great deal of interesting information about the institute and has contributed a unique argument in favor of moderation in American foreign policy.

RICHARD M. MILLS

Fordham University

The Imperialist Revolutionaries: Trends in World Communism in the 1960s and 1970s. By Hugh Seton-Watson. (Stanford, Calif.: Hoover Institution Press, 1978. Pp. x + 157. $6.95, paper.)

This volume may be regarded as a companion to the Hoover Institution's annual *Yearbook on International Communist Affairs* which has been praised for the richness of its data and faulted for avoiding systematic analysis (*Slavic Review* 29: 704–05; *The Russian Review* 34: 224–25; and *APSR* 72: 819). Commissioned to write a short volume that would try to identify main trends in communist affairs during the decade of the *Yearbook*'s publication, Seton-Watson has produced an essay whose lasting impression is more polemical than analytical.

Since the *Yearbook* is devoted to international communist affairs, Seton-Watson opens with an appropriate question: "Is there a world communist movement?" (p. 1) The answer is "no"; there is no longer in existence a single organized and disciplined world communist movement (p. 5). Instead we have essentially three political phenomena: a Soviet-led movement supported by Communist parties outside Russia proclaiming their devotion to Soviet policy; a Chinese-led movement looking to the theory and practice of Mao Tse-tung as its model; and a miscellaneous collection of Trotskyist and other heretical groups professing themselves to be Marxist-Leninists (p. 4). Placing these findings in historical context, the scholar of the international communist movement would be bound to note the demise of "monolithic communism" that served as a centerpiece of American cold war policy. One might conclude that since the unity and discipline of international communism have been shattered, surely the communist menace is not what it used to be. Not at all, according to Seton-Watson, because the three aforementioned groups share a common residue of Marxism-Leninism and regard "U.S. imperialism" as the permanent long-term enemy. Of course intracommunist feuds affect relations between communist parties and the West but to Seton-Watson the ideological factor makes it possible to attempt a meaningful discussion of "communists" in world politics.

On this infrastructure Seton-Watson proceeds to examine four main topics affecting communist fortunes: the attempts by parties to obtain power by peaceful means; national liberation movements in the Muslim world, Africa and Latin America; communism in power; and communism in the Far East and southern Asia. On a technical level, Seton-Watson's ability to evoke an image of the problems and fortunes of a particular communist party through a capsule history compels admiration. His terse summary of the Eurocommunists' dilemmas of policy, viewed through the prism of the Allende experience, is also nicely drawn (p. 52). For analytical purposes, though, Seton-Watson's three-part model of contemporary international communism leaves us where we were prior to the fragmentation of the movement and the Sino-Soviet split. The Trotskyists and other miscellanea never were part of the world communist movement and in any case were not of much importance. A Chinese-led movement patterned on a Mao Tse-tung model is problematical, given the changes occurring in China itself. Finally, the central phenomenon of communist parties linked to the Soviet state on the basis of their devotion to Soviet policy begs the question of the quality of their devotion, as the author himself observes in his remarks on the dilemmas of policy in Eastern Europe and their own attitude toward national defense. Unquestioning loyalty to the Soviet leadership may have gone out with Stalin. In short, Seton-Watson's model leads to a reductionism that was inherent in the monolithic model: manipulation of the data to underscore the threat of communism. Thus condemnation by the West European communist parties of the Soviet invasion of Czechoslovakia may have made the parties more popular, in which case it could he argued that the Soviet leaders strengthened their position in West Europe, contrary to conventional wisdom (p. 113). The small Portuguese Communist party "powerfully promoted the cause of communism and of the Soviet empire" by their contribution to Portuguese government policy that made possible the victory of the Liberation Front of Mozambique and the MPLA in Angola (p. 26). The communists are damned if they support their governments and damned if they don't.

If communism is judged weak now, it may well become a future threat. The communist parties in the Arab lands may be weak but the threat to the West is serious. Mass production of the intelligentsia and mass production of skilled workers to promote rapid industrialization, enthusiastically recommended by American or European advisers, may lead to mass production of revolutionaries. Though communist parties in Africa appear insignificant, "it would be wrong to dismiss communism as a force for the future" (p. 76). The potential had existed in Sudan and exists in South Africa, where the racial policies of the government might lead the non-black educated classes as well as the colored and Indian workers into the fold.

It is, however, the threat of the Soviet empire and the failure of nerve of the West, particularly of American policy, that is Seton-Watson's central concern. He deplores the "short-term hedonism" of the "political class" of professionals, business people, medium-rank officials, trade union hierarchs and mass-media manipulators as well as the collective guilt complex of Americans growing out of their treatment of the blacks, the Vietnam War and Allende which has combined to frustrate a "power elite," the top decision makers who "probably still retained more self-confidence and more will to keep power than appeared" (p. 141). He warns us in apocalyptic language of the possibility of the surrender of the "semibarbarous, industrially inferior, but militarily superior and politically ruthless Soviet empire" unless we mend our ways. It is an impassioned cry for an Imperial America to assume its historical obligations in the global contest with the communist Imperialist Revolutionaries.

BERNARD S. MORRIS

Indiana University, Bloomington

Political Socialization in Western Society: An Analysis From a Life-Span Perspective. By Barrie Stacey. (New York: St. Martin's Press, 1977. Pp. 176. $17.95.)

Stacey's book is a timely successor to Herbert Hyman's pioneering one of 20 years ago (*Political Socialization,* Glencoe, Ill.: Free Press, 1959). Hyman's book was an inventory of the then-extant literature on the topic, and so is Stacey's. Stacey's review begins where Hyman left off but it differs from Hyman's work in that it purports to adopt a life-span perspective and is not restricted to data from the United States.

Stacey's analysis of socialization in Western society is both thorough and critical. While it includes studies of the early 1960s, especially those written from a system's analytical point of view, his major emphasis is on work done in the late sixties and mid-seventies. This more recent work demonstrates convincingly that "socialization does not necessarily result in civilized values and behaviour or necessarily contribute to the maintenance of political stability" (p. 2). Instead, the overall impression to be gained from the review is that change in political orientations and fluctuating levels of affect for the political system are far more common than one would have suspected on the basis of earlier work. Stacey's book thus goes a long way toward dispelling the charge frequently levelled against socialization research, namely that it is essentially static and conservative.

Stacey also familiarizes the reader with a wide spectrum of literature possibly not familiar to U.S. readers, such as work done in Finland, Italy, and France—to name a few. Here, however, he seems to have restricted himself to work published in English. What is lacking therefore, to cite but one example, is a review of the substantial literature of the last two decades on German youths. Also notable for its absence is material on Western societies which are not democratic. For the most part Stacey seems to equate Western society with democratic societies and more specifically with those under Anglo-Saxon influence, or so it would seem, inasmuch as a preponderance of the reviewed studies deal with England and the United States. In part this no doubt reflects the current "state of the art."

More problematic is Stacey's claim to having adopted a life-span perspective. Such a claim would lead the reader to anticipate a consistently developmental perspective throughout the book. This, however, is not the case. For one thing, it is difficult to detect the specific developmental and/or life-time perspective to which the author subscribes. For another, there is a curious lack of integration from one chapter to the next. For example, in the first chapter he talks at length about the affective development of the child (spending an inordinate amount on the psychoanalytical transfer thesis) and all but ignores the child's cognitive development. Then in the next chapter, he deals in much detail with the adolescent's cognitive development but ignores emotional development. Development, however, is multidimensional; both the child and the adolescent develop cognitively and emotionally as well as physically. The chapter on radical youth repeats much that is known and dwells at perhaps too much length on non-empirical and frequently pseudo-psychological literature, but the data on youth's attitudes toward the Vietnam war do much to correct a prevalent image of youth as the vanguard of the antiwar movement. Unfortunately—the last chapter notwithstanding—childhood and youth remain the main focus of the book.

The claim to a life-time perspective, moreover, leads me to anticipate an equally thorough analysis of the changes which transpire in adulthood not only those connected with aging itself, but with occupational socialization, social and traumatic experiences, exposure to discrimination, to changing cultural norms. Such material is lacking. Curiosly absent are data on subgroups, especially racial and ethnic ones, and on the recent changes in the socialization of women. Despite these shortcomings, it is an achievement in itself that the author recognizes that socialization proceeds through the life process and carries it forward, however modestly, into adulthood.

The last chapter, oddly enough, extends the review to Third World and Eastern Bloc countries. It is unclear why this was attempted here when these countries were ignored in the preceding chapters. Unfortunately, the book ends here, thus leaving us essentially with an important review of the literature but no more. Missing is a concluding chapter in which the author poses the question: just what general conclusions can we draw on the basis of this overview? It is the virtue of Stacey's book that it offers the reader enough material to pose the question for oneself.

ROBERTA SIGEL

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The Politics of Agricultural Mechanization in China. By Benedict Stavis. (Ithaca, N.Y.: Cornell University Press, 1978. Pp. 288. $17.50.)

Stavis has written a useful survey of developments in Chinese policy regarding the mechanization of agriculture since 1949. This is an enormously important policy area, intimately related as it is to the key choices of the nation's economic development strategy, the building of rural bureaucratic and mass constituencies, and the real course of elite power struggles over the last 20 years. At various places in the narrative, some light is shed on each of these groups of issues, and on their inter-relationships, making it likely that parts of the book will be very helpful in teaching. However, the work's overall political analysis appears incomplete and, in places, obscure.

The presentation is chronological, from land reform through the cultural revolution, and Stavis is at his best discussing the earlier years. He performs an extremely valuable service in chapter 2 by gathering and evaluating the very wide-ranging estimates that have been put forward on deaths by violence during land reform. This question has provided ammunition for many emotional charges and countercharges, with figures running up to 14–15 million deaths (p. 26), and so it is important to have Stavis review the evidence dispassionately and to offer this tentative conclusion:

> It would appear that somewhere between 400,000 and 800,000 people were killed officially after 1949. . . . The Chinese Communist leadership had estimated that landlords and their families constituted 4–5 percent of the rural population–about 20 million people. This would imply that 1 to 4 percent of landlords' families met death. If a half million people were killed in land reform, this would be .1 percent of the rural population or 2.5 percent of the landlord class and would represent roughly one death in six landlord families (pp. 29–30).

Elsewhere Stavis considers it reasonable to assume that as many people were killed "unofficially" as officially, and so if he is correct, the total for the land reform period (1949–52), including non-landlords, may be somewhere over one million deaths. This is a credible estimate for purposes of comparison with agrarian reform movements in other countries. And in this context Stavis urges his readers to recall that the Chinese land revolution, often vicious and sometimes horrible, did nonetheless effectively put an end to a traditional rural order by no means usually humane or pacific, but itself heavily scarred by unreasoning terror and violence, both official and unofficial.

In the next chapter on the 1954–55 debates over agricultural collectivization, there is a good summary of the issues at stake at the time, and some interesting statistical material on contemporary social and economic trends illustrating the very hard choices faced by the leadership. Here Stavis also makes the important point, often not apprehended by previous students of the period, that the seemingly solemn invocation of the precedents of the Soviet model of agricultural collectivization and mechanization by all parties to these debates in Peking was neither naive nor doctrinaire, but consciously rhetorical. For Chinese leaders in the 1950s, the issue never was whether or not to follow Soviet experience to the letter. By that time, although they were still obliged to speak of that model in glorious terms, they were all very mindful of past Soviet errors and disasters. The issue they did face, as Stavis shows, was how, if at all, it would be possible to disentangle the positive and progressive elements of what the Soviets had done in agricultural collectivization from the politically and economically devastating mistakes they had made. For while the Soviet economy was in a state to survive mistakes of that order, Chinese leaders well appreciated that their economy was not.

However, at about this point in the study, a disconcerting vagueness sets in. The chapter on the organization of machine tractor stations leaves a surprising number of basic questions dangling. The section on the economic theory of the Great Leap is neither sophisticated nor complete. And the account of the post-cultural revolution reforms in mechanization policy touches only the high points, leaving the reader without a clear understanding of the complexities of making such a system work.

Part of the problem seems to be that Stavis has failed to consult any of the Chinese-language materials available on these topics. Despite the very impressive list of "Chinese Periodicals Cited" at the end of the book, it is in fact quite clear that the author has relied almost entirely on the various English translation series. This is adequate for some kinds of questions, but it cannot yield the depth of on-the-ground detail about policy implementation and reformulation that we expect in a good case study.

Another part of the problem is that Stavis consciously declines to adopt *any* model, or any combination of models, of the Chinese political process (p. 21) to assist in explaining policy change over time. The discussion leaps from the consequences of technological insufficiencies, to the vagaries of Politburo in-fighting, to the development of middle-level bureaucratic muddles. All are treated more or less equally as "constraints" on the process of China's agricultural mechanization, so that the reader is given almost no help in weighting the import of these various factors, and no clues for the construction of predictive hypotheses. The book is, therefore, more a chronicle than an analysis.

This is unfortunate, because the failure to develop an integrated outline of the political process prevents Stavis from achieving the major goal he sets for the work in the preface. He says there, "What I do hope can be learned from China's experience is that it is possible to impose political will over the path of technological development" (p. 8). But what his study actually illustrates is not clearly an imposition of political will over technological change, but a tortuous process of social and economic experimentation yielding a series of tentative, matter-of-fact, compromise solutions to complex problems only partially understood by decision makers and only partially responsive to their manipulations. The imposition of political will over the path of development may have made more headway in China than most other countries, and with Stavis we may hope that it will yet be decisive there. But the descriptive parts of his own work undermine our conviction in the matter, and this, when coupled with more recent development policy pronouncements emanating from Peking, must keep us skeptical of his book's opening assertion that "the honeymoon with technology is over."

VIVIENNE SHUE

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The Citizen and Politics: A Comparative Perspective. Edited by Sidney Verba and Lucian W. Pye. (Stamford, Conn.: Greylock, 1978. Pp. xv + 257. $14.95.)

This collection of essays, written in honor of Gabriel Almond by several of his students, close associates, and friends, offers ample, if sometimes indirect, evidence of Almond's wide-ranging contributions to the field of comparative politics. The substantive gamut spanned by the essays is so great, in fact, that the editors are hard-pressed to establish an integrative framework for the volume. Political participation, highlighting as it does the citizen's role in socio-political life, is the overarching theme eventually settled upon, yet the separate threads of the different essays fall short of fashioning any whole analytical cloth.

The essays are organized under three topical subheadings. Part 1, entitled "New Dimensions of Participation," contains three essays which clearly illustrate the broadening scope of inquiry concerning citizen participation. Whereas, as the editors note, earlier participation studies concentrated primarily on electoral activities and their systemic consequences, recent studies have frequently looked at more immediate concerns of citizens and the means employed to deal with them. Thus, in his essay "The Parochial and the Polity," Sidney Verba considers "parochial participation"—actions with narrow, highly particularized goals—and explores the foundations for both citizen perceptions of government as a potential provider for their needs ("parochial awareness") and citizen beliefs that government will actually provide needed services if called upon ("parochial expectation") in seven nations for which data are available. Findings from this inquiry are highly interesting and raise some vexing questions for the social service state since previous experiences with services rendered, especially if they are unsatisfactory, appear to exert a major influence on levels of parochial awareness and expectation.

The other two essays in Part 1 also look at narrower, more particularized forms of citizen activity and their implications. Wayne Cornelius investigates what he calls "demand-making" by the urban migrant poor in Mexico and finds that this activity, which is largely collective (springing from community improvement associations) but still highly parochial and limited in scope, displays little potential for producing a crisis-generating participation demand explosion, contrary to the expectations suggested by some theories of sociopolitical change. These findings corroborate Verba's claim that such parochially oriented activities have little bearing on other forms of political participation; despite their personal importance, these activities apparently do not furnish the basis for generalizing citizen political involvement. Finally, in an article devoted to an imaginative discussion of civil-court actions in Africa and the American South (citizen participation in what he calls "micro-policy formation"), Robert Mundt presents evidence which, due to its mixed character, calls into question several hypotheses concerning possible developmental relationships and sequences involving participatory structures and attitudinal orientations.

Part 2, labeled "Comparing Participation," encompasses four essays of a more varied nature. Lucian Pye, in his essay "Participation and Authority: A Comparative Perspective," argues that participation can only be properly understood in the context of prevailing attitudes toward authority and political leadership, attitudes which derive from basic socialization experiences. This argument is illustrated by comparison of Western and Asian participatory practices—the former characterized by conflictual relations, the latter by paternalism. On the surface these practices appear unreconcilable, yet Pye suggests they constitute quite natural and historically stable outcomes given the possible combinations of dominant orientations to authority and participation (which he sets forth in an analytical typology) and the salient socialization experiences within each cultural setting. A subsequent essay by Myron Weiner focuses on the concept of *citizenship* and the recent tendency for this term to be imbued with multiple meanings, particularly in multiethnic societies, in contrast to its "classical" universal denotation. This tendency is illustrated by reference to India where a constitutionally guaranteed unitary citizenship principle has been eroded by passage of various domicile rules protecting labor markets and educational opportunities for members of specific ethnic groups. India, Weiner correctly notes, is not unique in this regard, but whenever such developments occur, they pose a critical challenge to national political loyalties and integration.

In the most ambitious essay in the volume, Scott Flanagan returns to the age-old problem of "why some democracies 'work' and others do not" (p. 129). Following in the footsteps of Almond and Verba, Samuel Huntington, and Dankwart Rustow, whose work he makes a heroic effort to integrate, Flanagan argues that successful democracy is dependent above all else on the attitudinal orientations of citizens. He then goes on to suggest a general framework for explaining the genesis of three cultural variants observed in advanced industrial societies. Flanagan's major contribution here is to identify two alternatives to the civic culture—one a "combative culture" characterized by alienated parochial-participant orientations, the other a "spectator culture" characterized by passive parochial-subject orientations—both of which represent potentially unstable products of prior developmental sequences. France and Italy, it is suggested in passing, are primary examples of the combative culture, while Japan (and, one might add, West Germany) is seen as an illustration of the spectator culture, a case which Flanagan discusses at length using assorted survey research evidence.

The last essay in Part 2, by Joseph LaPalombara, is largely a continuation of an earlier polemic. LaPalombara's principal concern is the application of concepts originally formulated with reference to political phenomena in Western liberal democracies—terms such as "political participation" and "pluralism"—to Communist political systems in recent writing and the danger that such usage may leave these terms devoid of meaning. While this concern is legitimate, LePalombara does not adequately respond to the gauntlet he throws down for others; indeed it would seem he contributes to the very conceptual obfuscation decried in others when he ties political participation to pluralism (pp. 171–72). Why genuine citizen participation, even as LaPalombara defines it, can only occur under conditions of pluralism is neither logically nor empirically clear. The crux of the conceptual problem with political participation is that most citizen activities only achieve their political significance due to the motivational considerations associated with them. Yet, as LaPalombara indirectly acknowledges in an important aside (p. 172), political analysts can do no more than infer the political intent of most citizen acts. Until this methodological obstacle is overcome, and it may well prove to be one of several intractable problems confronting political scientists, the study of citizen participation will continue to be fraught with analytical ambiguities. Despite this weakness in LaPalombara's argument, his basic point remains valid—political scientists must be sensitive to the conditions under which citizen activity takes place—and his discussion of mobilization as a political technique contains a provocative prediction that merits further consideration.

Part 3 of the volume consists of three essays concerning the "Linkage of Citizen and State." In a joint essay, G. Bingham Powell and Lynda W. Powell focus on citizen-elite attitudinal concurrence, exploring new methodological approaches to this question and then applying the techniques discussed to citizen and mayoral opinion data from 48 Austrian communities. The strength of the Powells' approach is that it permits analysis of the structure of opinion representation, not merely the degree of representation alone. The findings presented, moreover, offer some noteworthy evidence regarding the efficacy of elections as mechanisms for transmitting citizen attitudes. Bernard Cohen's essay on "Citizen Participation in Foreign Policy" which follows is largely a retrospective and prospective overview of the topic, an attempt to assess what we know in fact and what remains to be discovered about the role of citizens in foreign policy making. Cohen's conclusion is that the present state of affairs is akin to an impressionist painting: general features are readily recognizable, especially from a distance, but specific details dissolve on closer examination. In response to this situation, Cohen prescribes a set of criteria which he believes will be useful in guiding further research on the subject and then elaborates upon these suggestions in a discussion devoted to the role of the press. The final essay is one by Robert Packenham on "Social Science and Public Policy" in which he addresses the role of social scientists as citizen participants. Packenham ranges over many issues, including whether social science is genuinely applicable to public policy making; if so, whether it is desirable; and again, if so, under what conditions should such participation take place. These normative questions, of course, do not lend themselves to easy answers. Rather than attempting to stake out any definitive positions, therefore, Packenham contents himself with a lucid discussion of some of the problems involved.

These remarks, to be sure, only skim the surface of the essays in this volume. In summing up, however, two observations are in order. First, readers will find that seldom have the efforts of scholars been subject to worse treatment in publication. Typographical errors are endemic to our profession and should perhaps be stoically endured. But in this book, such errors become epidemic—three or four egregious errors per page are not uncommon—and general editorial sloppiness appears to have achieved virtuoso status. Complete omission of Figure 1 from Mundt's essay, omission of narrative prose from Packenham's essay, an inability to alphabetize reference listings, and inappropriate page repetition are merely some of the more glaring examples of assorted transgressions. It is, therefore, only by virtue of the diversity of the issues raised and the quality of scholarship brought to bear that in spite of these distractions, the volume stands as a fitting tribute to the eclectic influence and inspiration Gabriel Almond's work has provided to others in the discipline.

LAWRENCE E. ROSE

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Sons of the Soil: Migration and Ethnic Conflict in India. By Myron Weiner. (Princeton, N.J.: Princeton University Press, 1978. Pp. xviii + 383. $22.50.)

India is a unique political system. Within that uniqueness there are common elements which it shares with other societies although these common features appear in special arrangements and permutations. The outstanding value of this book lies in its portrayal of the singular interweaving of universal with particular realities and the way it brings to light many problematics of political decision in fullness of detail against the backdrop of general principles. It therefore has value for two different audiences: those interested in the wider ranges of theory who look for ways to incorporate such insights into full-bodied particulars, and, conversely, those specialists in the study of India who have a keen interest in the ethnic conflicts of the subcontinent and who will be led by the author to see this subject as a special case with wider implications.

Since this *Review* is not primarily attuned to the concerns of the area specialist, I will begin with Weiner's focus on comprehensive issues embodied in his last two chapters. The major question he addresses is: when citizenship means equality for every full-fledged member of the national community but at the same time individual states are granted autonomy to enact their own laws freely, how much leeway do states have to protect their citizens from competition by migrants from other states with superior qualifications for positions in the upper echelons in the economy or polity? How much leeway are individual states granted to protect their often less-qualified members in the scramble for middle-level jobs? Since any local protectionism means, in practice, discrimination against migrants as migrants regardless of merit, it violates the migrants' equal status in the national context by forcing them to become unequal in the local context. An important assumption, of course, is that equality entails the citizen's right to move freely throughout the nation without being hampered by "artificial" restrictions.

Realistically, however, such circumstances ebb and flow with the employment market and while it expands or contracts *pari passu* with the economy as a whole, such differentials as industrialization, educational facilities, population characteristics create regional variables. It is a limited range of these regional conditions that occupies the center of attention in Weiner's work.

This is because of India's singular position among the developing nations. No other country in the third world has given as much linguistic autonomy to its component states as India. Combined with this is a national caste elite reinforced by a British colonial system which produced a burgeoning middle class spearheaded more by bureaucrats than by merchants and entrepreneurs. As B. B. Misra has emphasized (*The Indian Middle Classes,* London: Oxford, 1961, p. 251), development of the economy made its way from the top downwards rather than from the bottom upwards. The fiercest competition for jobs was therefore for positions in the middle range where lawyers, accountants, *babus,* bureaucrats, teachers and white-collar workers of all sorts inherited their superabundance from British practice. The royal road to such positions was higher education on the one hand and sponsorship from incumbents in the system on the other.

These conditions set the stage for the special type of ethnic conflict portrayed by Weiner. Typically, the locals with their own language, dubbing themselves "sons of the soil" now react vigorously against the influx of those from other states who speak a different language, have more education, and hence come to fill major positions in the economy, public administration, and the universities (where English is often the lingua franca). The indigenous people retaliate through legislative enactments that legalize protected labor markets (reservations) for home constituents, setting up preferential hiring practices that apply to a wide gamut of occupations. When feelings run high, there are public demonstrations and even riots or violence against outsiders.

Weiner presents three detailed case histories of such conflict, one in Assam, one in Chota Nagpur and one in Andhra Pradesh (the latter a special case where locals, called *mulki,* predominate in the Hyderabad area while the more privileged group migrate to the capital from coastal or near-coastal regions). A fourth case—Bombay—receives only minor attention. Each of the major cases is carefully explored in historical, economic, demographic, and cultural-linguistic terms. In addition, Weiner has done extensive field work with carefully focused interviewing so as to make the viewpoint of antagonists luminous and intelligible. Every careful reader of this volume will come to appreciate fully the insoluble dimensions of public policy revealed by Weiner's analysis and thus concur in his final statement: "One of the major questions for the Indian government is whether it uses its authority to build an internal common market, with special mobility as one of its features, or lends its support to those groups that want government to pursue internal protectionist policies. India seems likely to develop with either set of policies; the question is, what kind of India will it be?" (p. 371). While this judgment is premised on the assumption that one alternative will be chosen and the other rejected, the book as a whole makes it clear that problematic conditions will remain, regardless of choice.

One noticeable omission in the book is a discussion of the role played by caste nepotism in both higher education and the labor market—a mechanism enabling "outsiders" to establish a foothold and strengthen it. On the other hand, there are illuminating by-products of the research that are noteworthy such as mention of the affinity of locals for socialism rather than capitalism (p. 181) or the generalization that reserved jobs may do more harm in the private sector than in the public sector (p. 204).

Sons of the Soil is a splendid addition to Weiner's many volumes on Indian politics and a masterful analysis of nativism as an internal problem of governmental policy.

R. A. SCHERMERHORN

Claremont, California

Careers in Shanghai: The Social Guidance of Personal Energies in a Developing Chinese City, 1949–1966. By Lynn T. White III. (Berkeley: University of California Press, 1978. Pp. xiv + 249. $13.50.)

Lynn White's book on state-steered career patterns and citizen response in socialist Shanghai draws on the voluminous research White has done on this city over many years, beginning with his dissertation work. Its impact lies in its attention to three knotty issues, central to political philosophy, development studies, and planning in socialist states. As White deals with these issues, a theme emerges which centers around the limits on planning, especially centralized planning for cities, in a developing socialist society.

The three issues, then, are these: the relationship between political policy, economic productivity ("material supply," in White's terms) and mass compliance; the balance between the individual's own desires and communitarian benefits, and how the state can handle this conflict; and, finally, the nature of the interaction between state decisions, on the one hand, and "mass" or grass-roots action and reaction, on the other. In each case, we find that overall state-level political goals (perhaps best summarized in White's words on p. 206: "*increasing capital funds and . . . increas[ing]* the social efficiency of individual labor") are circumscribed, either by the level of economic prosperity, by individuals' preference for self-fulfillment and material gain, or by local responses that cause central planners to compromise in implementing their policies. This message of the checks on regime resolve, though left largely implicit, permeates the text.

White in fact spends most of his words in fleshing out these more philosophical concerns with his data. He does this by tracing state incentives and their popular effects over time (up to 1966) in four sectors in Shanghai, to each of which he devotes a separate chapter, and each of which he presents as a separate stage in an individual's life: education, the citizenship *rite de passage* of the youth-to-the-countryside (*hsia-hsiang*) movement, job placement, and residential controls. Here, through fascinating detail, we get a real sense of the individual aspirations and resentments connected with each policy sector, as we learn, not to our great surprise, that the Chinese of Shanghai and environs are enthusiastic about education, ambivalent (at best) about rustification, resentful if, as less-skilled and thus less-valued workers they are denied welfare perquisites, and that, if they are from the surrounding countryside, they are anxious to migrate into Shanghai and circumvent the registration and rationing systems whenever possible.

The state in China has attempted to find compromises and inducements to compliance —as by encouraging semi-autonomous spare-time schooling for workers and the unemployed when full-fledged facilities were lacking, and by promising future honors for present rusticated youth. However, after examining the fate of state efforts in directing individuals' urban residence and achievement patterns, White comes to an original and significant conclusion (pp. 216–27):

> Individuals had more low-level power and mobility when the government was preoccupied with commodity shortages, or also when real prosperity lowered the value of the material incentives that the regime could offer for socialist careers. . . . The state had most power in daily affairs and the citizens had least, when economic prosperity was medium.

Particular strengths of the book include its wealth of data and statistics on urban strategy in Shanghai over a 17-year period; its attention to continuities in policy over this period, as contrasted with the over-concern of many China scholars with shifts and cycles; its avoidance of rigid and hackneyed periodicization in decision-making analysis; its heavy reliance on the non-Communist press of the city, an unusual source in a field where governmental media are the data base for most work; and its effort to keep before the reader the perspective of the populace. Sections of the book that contain material difficult to find elsewhere describe illegal channels for subverting state controls, such as "underground factories," "spontaneous enterprises," black markets, and prostitution; and the conflict between state productivity concerns and the provision of welfare benefits. The especially useful chapter on residence includes insightful information on household registration, food rationing, urban transport and housing and birth control.

As is often the case with books that do a good job in presenting a particular viewpoint—in this case, convincing the reader of the limits on state direction—this volume leaves one wondering whether that theme might have been overstated. Thus, we finish with a view of a largely alienated citizenry (as White admits on p. 227) and of policy as very much a dependent variable. If these impressions are valid, how then are we to account for the very real change that has occurred in China over the past 30 years?

Also, three sorts of data, largely missing from this book, might have added to its value. White could have offered us a bit more background, both historical and political, on Shanghai itself, to help set the context for his story; he might have made more explicit the "developing society perspective" to which he refers on p. 10, perhaps by placing beside Shanghai's problems some comparative material on urbanization; and, though he adequately justifies a decision to discuss only the pre-1966 period (p. 4), it still would have added to the book had White noted expressly which of the social strains he identifies fed into the rhetoric and rebelliousness associated with the Cultural Revolution.

But, all said, White is to be commended for collecting, and then sharing with us this largesse of Shanghai lore, and for presenting it in a context of some sensitive and innovative theorizing.

DOROTHY J. SOLINGER

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The Russian New Right: Right Wing Ideologies in the Contemporary USSR. By Alexander Yanov. (Berkeley: Institute of International Studies, 1978. Pp. xvi + 185. $3.95, paper.)

Alexander Yanov's study of the resurgence of Russian nationalist thought analyzes both dissident and official nationalist ideologies and attempts to predict their impact on the future of the Soviet political system. His major hypothesis is that the "political meaning of contemporary Russian nationalism consists . . . in the gradual construction of an ideological base for the possible transformation of the regime in the USSR and the restoration of Stalinism" (p. 7). Yanov contends that (1) there are two major schools of nationalist thought in the USSR, the "dissident right" which originally identified with the political opposition to the regime and the "establishment right" supported by the regime, and that (2) although these two schools were originally in violent opposition to each other, they may eventually merge into a "military imperialist nationalism" in support of a future Stalinist type of regime.

Yanov argues that the Soviet leadership may well turn toward "military imperialist nationalism" because of its ostensible inability to cope with intractable foreign and domestic problems and in order to restore the system's capacity for mobilizing the society (p. 19). Yanov also asserts that the dissident right, unable to overcome the Westernism it perceives in the opposition movement and cut off from the masses by the government's control over the media, would repudiate its original support for political and intellectual freedom and endorse the "military imperialist nationalism" sponsored by the regime. While Yanov concludes that such a common doctrine of the Russian right has not yet emerged, he does believe that the dissident right has already shifted from its original liberal oppositionist stance to an "isolationist totalitarian nationalism striving for collaboration with the regime."

Whatever the validity of Yanov's prognosis (and his prediction is presented as a highly likely possibility rather than as the sole outcome of current trends), his discussion of the various strands of contemporary nationalist ideology is fascinating. His study stresses both the complexity and diversity of current nationalist thought as well as its striking resemblance to the Slavophil ideologies of the nineteenth century and the corporatist ideologies of the early twentieth century. Not only does he illustrate the nationalists' increasingly chauvinistic and messianic orientation, but he also attempts to unravel the conflict within the leadership which permits nationalistic diatribes to appear regularly in such official journals as *Molodaia Gvardiia.* In conclusion, Yanov maintains that the current leadership is ambivalent and confused in its approach to the revival of nationalist ideology. While it opposes the most strident and extreme appeals to repudiate Marxism-Leninism and adopt a purely nationalist, or nationalist-religious orientation, it has proved unwilling or unable to keep chauvinistic, anti-Semitic, and pro-Stalinist articles out of its journals or to resolve conflicts between the agitprop and cultural divisions of the Central Committee's secretariat over publication of these materials.

Yanov's survey is replete with historical parallels and some of them are very useful. His emphasis on the similarities between the current dispute between nationalists and political liberals in the USSR, on the one hand, and the Slavophil-Westerner debate in nineteenth-century Russia, on the other, effectively highlights the common preoccupation with the questions of Russia's "uniqueness" and the applicability of Western political institutions to Russian political culture.

Far less effective, however, is his effort to predict the nature of the post-Brezhnev regime within a cyclical theory of Russian history. Yanov contends that the current system may give way to a "Stalinist" regime on the grounds that Russian history has always been characterized by alternations between "soft" and "hard" authoritarian regimes. While there is no doubt that each successive regime in Russian history has been obliged to cope with the legacy of its predecessors, this process of adaptation hardly makes a return to the horrors of Stalinism the most likely possibility. Yanov seems to have an apocalyptic theory of historical development, to believe that Stalinism may return because of the Soviet leadership's inability to "solve" fundamental problems, and to reject the more plausible conclusion that the Soviet regime, like all other regimes in the world, does not even attempt to "solve" fundamental problems but simply muddles through.

Nevertheless, prognosis aside, Yanov's discussion of contemporary Russian nationalist ideology is truly essential for all those interested in any way in contemporary Soviet politics.

JONATHAN HARRIS

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International Politics

The Pentagon and the Making of US Foreign Policy: A Case Study of Vietnam, 1960–1968. By Jaya Krishna Baral. (Atlantic Highlands, N.J.: Humanities Press, 1978. Pp. ix + 333. $15.00.)

This book is an account of Robert S. McNamara's long, dark journey into the Vietnam night. The author, an able young political scientist from India, examines the relationships of the secretary of defense with the joint chiefs of staff, CINCPAC, the military command in Saigon, the State Department, the Central Intelligence Agency, the White House and the Congress. Occasionally, a few of McNamara's close civilian associates come into view, notably John McNaughton and Alain Enthoven, his assistant secretaries for international security affairs and systems analysis, respectively. Yet the principal figure always remains the energetic and controversial McNamara, at first in general accord with the larger policy community, later at odds.

Drawing heavily on the Pentagon Papers and on congressional hearings, Baral examines the defense secretary's role in selected historical episodes, e.g., President Kennedy's decision to send advisors to Saigon, President Diem's demise, the Tonkin Gulf affair, the start of the bombing campaign, and the introduction of American ground combat forces. This method of organizing the evidence requires Baral to lead his readers through some of these episodes more than once as he attempts to come to grips with McNamara's multiple relationships. As a result, the book is repetitive. It is also marred by some awkward word usage, careless proofreading, and the author's insistence on beginning each part of the argument with a dutiful recital of graduate seminar propositions, all dutifully footnoted.

But those who make it through to the third and fourth chapters, easily the best in the book, will find a number of rewards. First there are useful reminders, such as the observation that Hanoi's major support for the Viet Cong followed, rather than preceded, America's commitment to aid Saigon in the late 1950s. And that the Pentagon must have realized that the suspension of American aid would be interpreted by anti-Diem Vietnamese generals as a sign that we would not oppose a coup in 1963. Second, there are provocative guesses, e.g., that in easing McNamara out of office in 1967, President Johnson may have intended to soften up the joint chiefs of staff for his own eventual decision to de-escalate. Third, there is the unusual insight that can come to an intelligent observer from another country: the Sino-Soviet rift probably contributed to America's readiness to intervene in Vietnam by allaying its anxiety over a communist threat to Western Europe.

In a less obvious way, Baral's cultural distance from the events under view probably helps explain why his work, with all its surface imperfections, stands up well against, for example, Robert L. Gallucci's *Neither Peace Nor Honor: The Politics of Military Policy in Viet-Nam* (Johns Hopkins University Press, 1975). Gallucci strains to explain as much as possible in terms of bureaucratic politics. Baral, on the other hand, is content to use that framework where it appears to be helpful, for example in explaining why the air force preferred an "enclave strategy" while army generals set out to "search and destroy." But he goes on to insist that major policy was shaped by more fundamental matters, by nothing less than the play of mass opinion or national ideology upon the highest echelons of government. Such a realization, I submit, comes more easily to one who stands outside America's Lockean tradition.

Finally, Baral's work is a useful companion piece to Richard K. Betts' brilliant study, *Soldiers, Statesmen and Cold War Crises* (Harvard University Press, 1977). While Betts demolishes the myth of a monolithic military, Baral documents the disarray of the civilians. Indeed, his most important point is that as McNamara began to disagree with the joint chiefs, he also came into conflict not simply with President Johnson but with elders of the Senate and with ordinary Americans, who remained hawkish long after doubt and disillusion began to trouble the civilian leaders of the Pentagon.

LAURENCE I. RADWAY

Dartmouth College

Moderation from Management: International Organizations and Peace. By Robert Lyle Butterworth. (Pittsburgh: University Center for International Studies, 1978. Pp. vi + 140. $4.95, paper.)

This book is dedicated to the proposition that many attempts to evaluate the effectiveness of international organizations (IOs) are misguided because they are based on the assumption that, to be effective, IOs must acquire supranational authority. Robert Butterfield believes that IOs are more likely to exert a beneficial impact through a process of *strategic moderation.* That is, IOs can be effective just by providing opportunities for states to strengthen shared norms, perceptions, and habits of cooperation. Butterfield analyzes the United Nations, the Organization of American States, the League of Arab States, the Council of Europe, and the Organization of African Unity with the aid of a complex set of "operationally" defined concepts in order to determine the extent to which they have achieved strategic moderation.

I say "operationally" because the coding procedures used in this project were *not* "designed by a genius so they can be applied by an idiot." Rather, panels of experts reviewed the 166 conflicts that Butterfield analyzes, and made decisions regarding their "seriousness" (i.e., "how great a threat it posed to international peace and security"), and the degree of impact that the IOs had on them.

Inevitably, in such a process, the experts will make some decisions of debatable validity. For example, in their review of the cases involving the UN, the experts ranked the Soviet-backed coup in Czechoslovakia and the Soviet blockade of Berlin (both of which occurred in 1948) as among the least serious cases in the UN's history (p. 34). More serious in their eyes were, for example, the disputes between Rwanda and Burundi in 1963–64, and the Adeni-Yemeni border dispute in 1964–66 (p. 45).

Butterfield and the panel of experts also have a disconcerting tendency to attribute effectiveness to IOs on the basis of widely disparate kinds of evidence. If the number of disputes with which an IO must deal *declines* over time, Butterfield interprets that as a sign that the IO has successfully managed conflict in its domain, which seems fair enough (p. 75). But if the number of disputes that an organization deals with *increases* over time, that too is interpreted by Butterfield as an indicator of effectiveness, because the organization "has been kept relevant to these problems" (p. 43). And if the UN fails to inscribe on its agenda the case of "Chinese aggression in Tibet," that is also interpreted as a sign of effectiveness, "because the act of noninscription itself was seen as a management effort" (p. 30).

A skeptic might suggest that this combination of debatable coding decisions and determined optimism accounts for some rather surprising conclusions by Butterfield. He reports that the UN, the OAS, and the OAU (but not the Council of Europe and the League of Arab States) have been quite successful in their efforts to achieve strategic moderation. More intuitive analyses of these organizations have come to more pessimistic conclusions. That could be because, as Butterfield argues, previous analysts have been looking for the wrong things, or because Butterfield has found evidence that is not visible to those who rely on less scientific procedures. Still, I suspect that some readers of this book will ask themselves: "Are these findings nonobvious, or obviously ridiculous?"

The key question that Butterfield addresses here is: "What would have happened if these IOs, which did exist, had not existed?" That is a useful question, but any attempt to answer it will confront terribly complex epistemological and methodological problems. That Butterfield and his colleagues have not solved those problems is not surprising. They have made an intelligent and conscientious effort to deal with them. That they chose to do so in an "operational" manner leaves them wide open to pot shots like those in this review; an analysis of these IOs based on more invisible procedures would have been less vulnerable to criticism. I believe, however, that such an analysis would have been less valuable than *Moderation from Management.*

JAMES LEE RAY

University of New Mexico

Island China. By Ralph N. Clough. (Cambridge, Mass.: Harvard University Press, 1978. Pp. x + 264. $12.50.)

There are very few good books about Taiwan, and not very many on the international politics of the Far East. There is ample reason, therefore, to welcome this excellent one sponsored by the Twentieth Century Fund and written by a distinguished retired American Foreign Service officer with extensive experience in the Far East (including Taiwan), a fine professional reputation, and a list of significant published writings. Although written shortly before the normalization of relations between the United States and the People's Republic of China, it is by no means obsolete or invalidated.

With unusual knowledge and objectivity, Clough analyzes the history and current status of Chinese Nationalist rule on Taiwan, the island's flourishing economy, Peking's attitude and objectives with respect to Taiwan, the bizarre only-in-China contest between the two unequal sides, and the island's unique international position including the interests and role of the United States, the prospects for normalization of relations between the United States and the People's Republic of China, and the likely impact of normalization on Taiwan. Since much of the information and analysis is derived from the author's personal knowledge and experience or from sources not readily available in the public domain, the general thinness of the documentation is both understandable and unimportant.

Clough's conclusions are compatible with what has happened since the publication of his book and with the Carter administration's Taiwan policy. In other words, he favors normalization with some safeguards for Taiwan's future security, especially the continuation of American arms sales to Taiwan, and he believes that Peking will accept, or can be brought to accept, such safeguards. In fact, of course, Peking did agree, evidently on account of alarm over the Vietnamese attack, with Soviet support, on Cambodia in early December, 1978, to ignore (not really to accept) the announced American decision to resume sales of "selected defensive" weapons to Taiwan, with no veto for Peking, after the expiration of the security treaty at the end of 1979. Most of Clough's colleagues would be happy to have come so close to an accurate prediction of the outcome of such a complex and difficult process.

Like most proponents of normalization, in the strict sense of establishing formal diplomatic relations as distinct from merely improving relations, Clough is better at explaining that normalization has been an announced objective of American policy since 1972 than at explaining why it is in the national interest of the United States. On the latter score, he cites the American commitment to normalization (in the Shanghai Communique), the presumed contribution of normalization to international stability in the Far East, and the possibility of deterioration of the pre-existing relationship between the United States and the People's Republic of China in the absence of normalization. None of these arguments is unanswerable, and even taken together they are not necessarily conclusive; like most major policy decisions, normalization was an act of judgment and a gamble. Peking's attack on Vietnam so soon after normalization can hardly have been pleasing to American advocates of normalization and has certainly not encouraged confidence in Peking's willingness to refrain from the use of force against Taiwan.

As for Taiwan itself, Clough advocates avoidance by the United States of pressure on Taipei to seek an accommodation with the mainland. He believes, however, that such an accommodation is likely to develop gradually in some form or other, even in the absence of such pressure. Perhaps so, but it is worth noting that normalization has deprived Taipei of much of its bargaining power vis-à-vis the mainland.

Although Clough discusses, fairly if somewhat briefly, the critical question of mainlander-Taiwanese relations on the island, he does not deal at any length with one of the most interesting aspects of normalization: its potential for exacerbating the rather difficult relationship between the mainlander establishment and the Taiwanese elite. The course of this relationship may have a profound effect on the future of Taiwan. It might lead, for example, to the proclamation of a Republic of Taiwan (acronym: ROT), which would create a knotty problem not only for Peking but for those numerous foreign governments, including that of the United States, that have officially stated that there exists only "one China."

HAROLD C. HINTON

George Washington University

The Making of United States International Economic Policy. By Stephen D. Cohen. (New York: Praeger, 1977. Pp. xxiv + 208. $18.95, cloth; $6.95, paper.)

The importance of U.S. international economic policy has increased dramatically during the post-World War II era. International trade has grown more than 50 percent faster than total domestic economic growth. Domestic economic departments in the federal government have been hearing very clearly from their constituents that U.S. international economic policy must reflect the latter's growing vulnerability to the external sector.

As a result of these developments, Steven C. Cohen has written a timely and surprisingly comprehensive study of the decision-making process in the U.S. international economic policy. The book is carefully written and well researched. The study is written from an "insider-outsider" perspective. Cohen was a recent staff member responsible for international economic policy to the Commission on the Organization of the Government for the Conduct of Foreign Policy. He is now on the faculty at American University.

A Washington anecdote, which the author discusses, illustrates just how important Cohen's topic is. Certain professional staff members of the Senate Finance Committee in 1976 proceeded to visit key executive branch offices and ask what the international economic policies of the U.S. were. "They heard not about specifics or how policy was made," the author writes, "but rather contradictions, disavowals of responsibility, equivocations, and criticisms of other agencies" (p. 204).

Cohen divides his analysis into three parts: "Substance," "Procedure," and "Prescription." Several of the individual chapters are especially well done. Cohen convincingly shows, for example, the significant impact which organization has on policy in chapter 3. He concentrates on the interaction within and between competing bureaucracies; chapter 4 traces the ascendancy of the Treasury Department in foreign economic policy. In chapter 8 Cohen analyzes how America's major economic partners go about organizing their own international economic policies.

In his discussion of the requirements for the optimal system for the making of U.S. international economic policy, Cohen notes that there is no such thing as the single perfect or utopian system. His proposal for a quick, well-defined, well-greased optimal organization is not matched by a persuasive discussion of how the necessary reforms can be brought about.

THOMAS N. THOMPSON

University of Oregon

Arms Across the Sea. By Phillip J. Farley, Stephen S. Kaplan, and William H. Lewis. (Washington, D.C.: The Brookings Institution, 1978. Pp. x + 134. $7.95, cloth; $2.95, paper.)

Any book published by the Brookings Institution demands attention—as would any book by any one of the three authors, all of whom are knowledgeable and experienced policy analysts and/or policy makers. Moreover, the subject of their work, the arms trade in all its aspects, is both important and timely. It would, therefore, be gratifying to be able to commend this book to the student and teacher of security policy. Alas! I cannot do so.

One reason for this is that the book is far too short for what it attempts to cover: the world arms market; the various forms of U.S. security assistance (private and governmental sales; credits for arms purchases; military aid, education and training; supporting economic assistance; co-production, etc.); the history of these programs over the past 30 years; the criteria governing their use; the details of their application; and recommendations for the future. The result is that important issues such as security supporting assistance are covered in a paragraph and both policy analyses and policy recommendations all too often consist of sweeping generalizations.

A second reason is that the book, despite its brevity, is amazingly repetitious; thus, rationales for security assistance are covered not only on pp. 26–27 and 34–35 but also on pp. 39–41 and 104. More importantly, chapter 5, on the application of arms policy to participating regions and states, not only repeats, in greater detail, much information given earlier, but also is itself partly covered again in chapter 6, which outlines policies for the future.

The third and most important reason is, however, that the book puts forward both contradictory findings and conflicting recommendations; for instance, in the middle of p. 91 the authors call for a reduction of dependency deriving from U.S. security assistance, while on the bottom of the same page they recommend as an alternative economic aid, which may simply substitute one form of dependency for another. Similarly, they call (correctly) for U.S. restraint on "arms sales that might upset . . . [regional] stability" (p. 127), while at the same time sponsoring continuing arms aid to Israel—which some readers, if not the authors, might view as potentially destabilizing. Finally, they argue (persuasively) for measures to reduce U.S. military assistance while noting that this would not be viewed "by the Soviet Union and its arms-producing allies, or France, or other nations in the West as a model to be followed . . . [but] as a unique opportunity to obtain new influence and customers" (p. 50).

This does not mean that *Arms Across the Sea* is without value: it presents some informative data, it dispels myths about the levels of arms purchases by most Third World countries, and it gives a good summary of U.S. policies, both general and applied. Moreover, its trenchant critique of these policies seems justified by subsequent events as well as by those it examines, and it raises a number of interesting suggestions as to future U.S. programs. One would, however, have expected Farley, Kaplan and Lewis—and the Brookings Institution—to do somewhat more than that.

J. I. COFFEY

University of Pittsburgh

The Foreign Policies of West European Socialist Parties. Edited by Werner J. Feld. (New York: Praeger, 1978. Pp. xi + 149. $16.95.)

Werner Feld hoped his anthology would inform the scholar, foreign policy analyst, and interested lay person about an important but neglected topic—the foreign policies of European social democrats (see p. 3). While I agree with Feld that these policies, their roots, and their implications are important and have not received the attention they deserve, his volume does not make the contribution that he—or I—would have liked.

An anthology like Feld's should, at least, have the following characteristics. First, the various contributing authors should test a theory or at least answer a common set of questions in their chapters. Without one or the other, neither the reader nor the editor can confront theory with data, integrate the material presented, and substantially improve our understanding, in this case, of European socialism. Second, each substantive chapter should be well researched, well documented, well written, and well organized. Unfortunately, Feld's volume fails on both counts.

In the introduction, Feld realizes that the most important task of his volume is to document how and then to ask why the European socialists' foreign policies differ dramatically. But, because he did not impose either a theory or a common agenda on his contributors, neither he nor they can really grapple with these issues.

Ironically, the germ of such a framework can be found in the substantive chapters. At one point or another, the contributors discuss the effects of a party's size, its political situation (whether or not it is in or near power), its alliance possibilities, its internal divisions, and the domestic and international political contexts it finds itself in. Each of these factors clearly has an influence on the various socialist parties' behavior, but because there is no common focus to the articles or the volume as a whole, we know little more about the differences in party behavior at the end that we did at the beginning of the book.

What we are left with, then, is a series of independent articles on the socialists' foreign policy in the United Kingdom, the German Federal Republic, Sweden, Norway, France, Italy, and Portugal; introductory and concluding chapters by Feld; and a brief analysis of social democratic history by George Windell. Had these been strong chapters, the book might still have been rather successful. But, alas, this is not generally the case.

Feld's introduction hints at a number of the key issues some of the contributors deal with, but by no means sets a coherent agenda. Moreover, he makes hordes of unacceptable substantive errors, as, for example, misspelling the name of French Socialist leader, François Mitterrand (p. 2) and mistakenly placing the death of the SFIO in 1971. His conclusion on international cooperation among European socialists considers an important new phenomenon, but does so too briefly and without any connection with the rest of the volume. Windell's chapter on the evolution of European socialism also deals with an important topic, but, it, too, has little to do with foreign policy, let alone the other articles in the book.

Yet, this book is not an abject disaster. While few of the substantive chapters break new ground, are based on original research, or deal with many issues in much detail, all will interest the interested lay person, if not the scholar of European socialism or the foreign policy analyst. The chapters on the lesser studied, smaller nations—Sweden, Norway, and Portugal—are the most thorough, insightful, and informative.

Still, when all is said and done, this is a frustrating book, both because it need not have been and because European social democracy has been so neglected in recent years.

CHARLES HAUSS

Colby College

Recognizing Foreign Governments: The Practice of the United States. By L. Thomas Galloway. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1978. Pp. xiv + 191. $4.75, paper.)

L. Thomas Galloway articulates the change in U.S. recognition policy that has been quietly emerging in recent years, namely the trend to eliminate, in most instances, the act of recognition of foreign governments. From his case-by-case examination of U.S. responses to governments that came to power through extraconstitutional means between 1961 and 1976, Galloway correctly concludes that today, unless the U.S. perceives a major policy interest to exist, such as in Angola and Cambodia, it will de-emphasize recognition and follow what amounts to the Estrada Doctrine. His study records the growing tendency of the U.S. government to take the position that no question of recognition arises (or that relations are normal or are continuing), thus removing such sensitive issues as whether the act of recognition or nonrecognition constitutes U.S. approval or disapproval of a new government. This policy, while creating a "conceptual muddle" (p. 146), has generally worked well, he contends, whereas efforts to promote political objectives by withholding recognition have largely been ineffective.

Benefiting from information gathered as an extern with the State Department's Legal Advisor's Office in 1972, Galloway, a public interest lawyer at the Center for Law and Social Policy in Washington, presents a brief, historical overview of U.S. practice from Jefferson through Eisenhower. Then, grouping states regionally, he analyzes each administration's response to *coups d'état* and revolutions through Gerald Ford. One chapter centers on the recognition practice of other states, using information provided by U.S. embassies in response to a State Department communication in 1969 and to the author's follow-up inquiry in 1975. In a concluding chapter, Galloway recommends that the Department of State *officially* announce that it has decided to eliminate the concept of recognition of foreign governments from U.S. diplomatic practice. The book includes a foreword by R. R. Baxter, the detailed responses of foreign governments about their recognition policy, and analytical tables on criteria, time span between events, and patterns of consultations. Analyzed also are doctrines which have been prominent recently, including the Estrada Doctrine, the Tobar or Betancourt Doctrine, the 1964 Mann Doctrine, the 1969 Cranston Resolution in the Senate, and the 1965 Rio Resolution providing for hemispheric consultations.

The book's major contribution is its analysis of *practice*, including some insight based on behind-the-scenes information; this raises questions about generalizations which appear in existing literature on recognition. Galloway shows that from the time of Jefferson through 1960, "U.S. policy varied from a *de facto* policy of almost automatic recognition to the active use of recognition to achieve policy goals" (p. 42). Thus, the application of the traditional criteria—effective control, consent of the people, and willingness to comply with international obligations—changed and was a source of disagreement and confusion among and within varying administrations. Furthermore, the author's data on foreign governments' practice shows that 30 of the 100 states surveyed follow the Estrada Doctrine or something similar, with others following it without being familiar with its name. This is contrary to Ernst B. Haas' observation in a 1969 essay which rather casually examines U.S. recognition policy (in *U.S. Foreign Policy,* Paul Seabury and Aaron Wildavsky, eds. [New York: McGraw-Hill, 1969], p. 75).

A strength of the book—the author's access to State Department records—has been converted (perhaps unavoidably) into a twofold weakness. Galloway fails to identify consistently the portions of his analysis which are based on these materials (it is probably safe to assume that unattributed footnotes are a clue, e.g., Guatemala, 1963; Peru, 1962; Argentina, 1966 and 1971; and Bolivia, 1969); and there is no precise reference to cases from which generalizations are made. These omissions are evident in Galloway's interesting description of the techniques used by press officers in dealing with persistent reporters. "Only if pushed hard, with no avenue of retreat, would the press officer announce that recognition had occurred. And even when this was conceded, the press officer was cautioned not to mention a particular act or point in time when recognition was accorded" (p. 146). Also, in reviewing Latin responses to the Rio Resolution which calls for consultation prior to recognition of governments established by illegal means, Galloway describes some consultation as "perfunctory" and others where "a good faith effort has occurred" (p. 133). In places, Galloway's observations tend to exaggerate. For instance, he writes that "Peru (1962) serves as the best example of the use of recognition and financial aid as bargaining levers to obtain a promise to return to constitutional government through free elections" (p. 49), yet in later paragraphs acknowledges that the U.S. was forced to back down. In the final chapter, Galloway departs from an analysis of practice to respond to arguments advanced against a policy which ducks the question of recognition. Here he only begins to examine the problems which may arise, such as situations where there are rival claimants to power or sustained political stalemates. He raises pertinent issues which should be explored at length by other scholars, and this was probably his intent.

In spite of certain limitations, some self-imposed, one should acknowledge the fine contribution Galloway makes in providing a record of actual practice in an overlooked area in U.S. diplomacy.

JOAN KRUEGER WADLOW

University of Nebraska, Lincoln

Latin America and World Economy: A Changing International Order. Edited by Joseph Grunwald. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 323. $18.50, cloth; $8.95, paper.)

This collection of original essays comprises the second volume in the Latin American International Affairs series sponsored by the Center for Inter-American Relations in New York. Twelve chapters and two commentaries by 17 authors deal with the changing character of Latin American economic relationships with developed countries, the rest of the Third World, and among themselves. The mixed nature of the collective authorship (including economists, political scientists, and historians, eight of whom are North Americans, seven Latin Americans, and two Europeans) is salutary. It helps ensure a broad spectrum of perspectives, to include, refreshingly, Latin American perceptions of their own problems and relations. Joseph Grunwald and his coauthors reasonably view the Latin American region as an important and manageable level of analysis while recognizing its heterogeneity. Grunwald sums up Latin America's global economic position with these words: "The poorest countries are among the poor of the Third World, the wealthiest could well be classified among the developed First World" (p. 7).

Virtually all multi-authored works seem to suffer to some degree from scholarly inconsistency and loose conceptual ties among the various contributions. In this instance the quality of effort varies and some discontinuities exist. Furthermore, certain important problems deserve more attention, such as those related to multinational corporations, dependency, and the communist world. But these difficulties are minimized, partly because the topical coverage is generally well conceived, all of the chapters are written for this volume, and Grunwald's opening essay largely succeeds in drawing together the total enterprise.

The opening chapter is a set of "reflections" by the editor on Latin America in the world economy, setting out the major issues, and concluding that Latin American governments will continue to fight within the Third World for the New International Economic Order. Four subsequent chapters analyze Latin American economic relations with industrial countries. Albert Fishlow's provocative criticism of U.S. policy focuses on restructuring trade, foreign investment, and capital flows. Laurence Whitehead is convincingly pessimistic about Britain's economic prospects in Latin America, foreseeing little increase in trade or investment. Albrecht von Gleich's judicious chapter leaves a picture of declining but still significant trade relations between the Federal Republic of Germany and Latin America, and of inadequate negotiating machinery between the European Community and the region. Claudio Velez argues much too briefly that trade between Latin America and the Western Pacific will almost certainly increase; in my view, he is convincing only with regard to Japan.

Mexico and Brazil receive special attention in separate contributions. A short chapter by Leopoldo Solis on the external sector of the Mexican economy is informative but largely descriptive. Werner Baer and Carlos von Doellinger divide Brazil's foreign economic policy since World War II into two periods, as its concerns shifted from traditional free trade and inflation fighting to incentives to increase and diversify exports. Three further chapters are devoted to regional integration and economic relations with the Third World. Ricardo Ffrench-Davis argues that the Andean Pact is a model of economic integration for developing countries; his solid treatment is more optimistic than the facts of intra-group conflicts would seem to warrant. Clark Reynold's treatment of Central American economic prospects is a sophisticated economic analysis that falls short of sharply linking politics and society to economics as he sets out to do. Felipe Herrera, in a well-informed and provocative analysis of Latin America in the Third World, is too sanguine in his views of the latter as coherent, united, and effective in international councils.

Two major issues are confronted in separate chapters. Luis Escobar addresses external financing in Latin America from a technical administrative perspective. Douglas Bennett, Morris Blachman, and Kenneth Sharpe coauthor a balanced and dispassionate analysis of Mexico and multinational corporations, emphasizing that both operate within important constraints. The book ends with two brief commentaries. Bryce Wood's nicely crafted piece asserts that the Linowitz Report of 1975 denies the usefulness of historical experience in the formulation of a new U.S. policy but actually restates the substance of President Franklin Roosevelt's Good Neighbor Policy. Gabriel Valdéz S. offers useful descriptive and analytic notes on the new Latin American Economic System (SELA).

Grunwald and his colleagues demonstrate the intimate relationship between economics and politics in international affairs. They clearly illustrate the considerable economic content of today's international politics, and amply reveal economic problems that may be described as political in nature.

G. POPE ATKINS

United States Naval Academy

The European Parliament and the European Community. By Valentine Herman and Juliet Lodge. (New York: St. Martin's Press, 1978. Pp. ix + 199. $19.95.)

In their preface the authors of *The European Parliament and the European Community* note that the book "grew out of a conversation over wine in the cafe of the European Parliament. . . ." The book, like many vintages of the Moselle, is best appreciated when sipped. The organization of the text and of its component chapters is highly concentrated. The authors know the European Parliament, and other European parliaments, well and assume that their readers are not beginners.

The book is divided into two major sections and has ten chapters overall. The first section is a careful description and analysis of the current (pre-election) powers of the EP. That section is done with consummate skill and with a verve not frequently seen in legal-formal writing. The second section opens with a review of the notion of legitimacy as viewed in the West and then turns to a consideration of the implications and difficulties of a future (post-election) EP. Specifically, they turn to an examination of public opinion, the involvement of the media, the future of the dual mandate, and the possibility of a bicameral European Parliament.

The authors' view of the EP as it is currently structured takes as its point of departure a current paradox that has surrounded the agonizing decision(s) leading to the promise of direct elections to the EP. On the one hand, some have argued that, since the body is so weak, Europe-wide elections would be a waste of time and effort. On the other hand, the argument is made that without elections the EP has no justification and no possibility for increased powers. As the authors note, it is a chicken-and-egg standoff. The arguments about what should come first, direct elections or enhanced powers for the EP, though, comprise only the surface aspect of the real problem.

One major concern is based on the notion that political integration of the Community cannot continue (or recommence) without direct popular input—one cannot integrate democracies without democracy. But equally bothersome is the fear in some quarters that participation in direct elections will transfer sovereignty and allegiance to a supranational unit at the expense of national parliaments and member states. The authors treat these concerns with sensitivity—they create no villians and no heroes from European federalists or nationalists. Yet they suggest that the immediate impact of direct elections is easily exaggerated. They argue, relative to the question of the transfer of sovereignty, that there really is not a "European citizenry" that "wishes to give expression to a European identity transcending the nation-states." Furthermore, any growth in the powers of the European Parliament at the expense of Community institutions like the Council of Ministers or national institutions like parliaments "can be almost indefinitely postponed."

The evidence which Herman and Lodge bring to bear in support of these points is largely contained in the first half of the book. After an opening review of the status of parliaments in democratic theory, and a nod to the "decline of parliament" thesis, they turn to a painstaking analysis of the powers and functions of the European Parliament.

It is not opportune in a brief review to trace the development of their reasoning in any detail. Still, to give a broad overview, after examining the legislative, financial, and control powers of the European Parliament, Herman and Lodge conclude in chapter 5 that direct elections to it should not be greatly feared (or welcomed, presumably) because the European Parliament is not a parliament. In fact, they do not merely conclude that it is not a parliament—they give no fewer than nineteen reasons why it is not.

If the European Parliament is not really a parliament, what then is it—or might it become after direct elections? This is the concern of the second half of the book. Herman and Lodge open their response to the question by noting that direct elections will both enhance and exacerbate the legitimacy problems of the European Parliament and Community institutions. Elections clearly add an aura of democracy to the Community institutions, but at the same time they will underscore the intangibility, unintelligibility, and virtual invisibility of the European parliament—in other words, it amounts to a body with symbolic input but neither specific nor diffuse outputs. Most serious of all in the authors' view is the fact that the European Parliament does not promise to behave as a "grand forum" for the European idea.

The second half of the book is devoted to the theme of the European Parliament as a grand forum in the sense of an education device for the European masses. Their argument is detailed, complex, well documented, and in touch with good political sense. Herman and Lodge elaborate on the requirements of "European" parties, devote an interesting chapter to the European media, and elaborate on the enormous difficulties created by the dual mandate of MEPs. Finally, they suggest a restructuring of the Community's institutions from a bicephalous executive (Commission and Council) to a bicameral legislature (Parliament and Council).

However, some students of parliaments in general, and some students of European integration may take exception to the authors' rather single-minded devotion to a European Parliament that serves as a tool to educate European masses—a grand forum of socialization. That is, no real attention is devoted to a European Parliament as an aggregator of popular or corporate interests. Nonetheless, this text's numerous virtues outweigh any vices and it is required reading for anyone interested in a parliament that is most definitely not in decline.

JOHN K. WILDGEN

University of New Orleans

The Nation-State and the Crisis of World Politics. By John H. Herz. (New York: David McKay, 1976. Pp. vii + 307. $5.95, paper.)

John H. Herz is one of a small group of European-American scholars who have played a decisive role in the emerging study of international politics in the United States. Herz's importance is less easily defined than that of Hans J. Morgenthau, Karl Deutsch and Arnold Wolfers, with whom he associates himself in a lengthy autobiographical introduction to this work. That he should feel compelled to write his own *Festschrift* is in itself revealing. The fundamental question inescapably arises in the mind of the friendly critic: is Herz a first- or second-rank political thinker?

If Herz had devoted himself unreservedly to international politics, his stature as a major figure almost certainly would have been proportionately greater. More than half his writings fall within the field of comparative politics and he is co-editor of the journal *Comparative Politics.* His formation in his native Germany and at the Geneva Graduate Institute of International Studies was in law, not politics, and his mentor was Hans Kelsen, founder of the "Pure Theory of Law." The rise of Hitler constituted for the young Herz "a brutal awakening" and opened his eyes to the role of power in international affairs. His first book (published under a pseudonym) on *The National Socialist Doctrine of International Law* warned of the deception of such writings in obscuring the warlike intentions of early Nazism. Herz's disillusionment with the world had its origins in the failure of collective security, the destruction of socialism in Russia on the anvil of Stalinism and the triumph in Germany of Nazi totalitarianism.

Yet Herz's disappointments failed to obliterate his inborn idealism and the indestructible romanticism which have characterized all his subsequent writings. In the first of a series of essays brought together in the present volume entitled "Power Politics and World Organization," he sought to delineate the "in-between system" between the anarchy of power and an unrealizable world government. His continuing blend of realism and idealism was expressed in one of two seminal works by which he must be judged: *Political Realism and Political Idealism: A Study in Theories and Realities* published in 1951 but written in the interwar period. Its significance lies more in the value of certain original political concepts, for example, the security-power dilemma, than in his political prescience (he acknowledges his failure to foresee the emergence of a bipolar world or the creation of nuclear weapons). His second classic work, *International Politics in the Atomic Age* (1959), not only remedied the deficiencies of the first book in describing postwar realities but added new concepts, for example the "impenetrability" of, first, the territorial state and, in the cold war, the postwar blocs of "client" and "satellite" states. He also set out to describe the strategies of nuclear deterrence thrown up to avert the threat of nuclear annihilation. Taken together, these two books of the 1950s from which valuable excerpts are reproduced in *The*

Nation State brought Herz to the threshold of joining other "Master Thinkers in International Politics."

Yet Herz's lasting influence as a theorist has been somewhat marred by three limiting factors. First, he represents, as he has said, the lonely individual scholar at work in a private intellectual laboratory, seldom if ever interacting with other theorists, including those who share his commitments and to whom he may even be indebted. Second, anyone who traces the evolution of his thought is impressed by a tendency to embrace utopian liberal attitudes (he once described himself as a liberal realist) not self-critically or dialectically as Reinhold Niebuhr did, but with his own brand of romantic enthusiasm. Third, Herz's capacity to shift his ground and adjust his thinking to new realities is both a strength and a limitation.

If collective security and problems of the nuclear age as they affect the rise and demise of the territorial state represent the first two stages of his thought (two exceptional articles on appeasement and detente must be mentioned here), his discussion of science and technology and the acceleration of developments in all fields of international relations constitute the final stage of his thought. Not only has attention shifted to the novel problems of food and population, environmental and resource depletion and "survival ethics" in this stage (an area of concern rather consistently overlooked by most political realists) but he also brings the power of a *Kulturkritik* to these vital areas, approaching them on the broadest humanistic front. He postulates that a "minimum ethic of survival" must supercede past reflections on individual or group ethical standards as evidenced in historic international politics. Yet he despairs over the malfunctioning of the nation-state system and the obstacles to realizing "survival ethics" in most foreign policy decisions, especially in the United States. New concepts are introduced, such as external and internal legitimacy, particularly for the 100 or more fledgling states. America's survival is threatened by its internal divisions into two societies, the active and inactive (the underclass or welfare groups) societies and by wasteful consumerism and selfish domination by the establishment. The haunting specter of democracy losing out to "benign authoritarianism" or a "friendly fascism" becomes a growing preoccupation for Herz. He is concerned with the "power surplus" of the great powers, their short-sighted compromises with popular demands for nuclear and industrial supremacy and mankind's unwillingness to contemplate without wincing the possibility of the end of civilization (he calls for reflection on the "ethics of a decent demise" or preparing ourselves for an exit from survival).

Herz's intellectual journey along the pathway of first seeking to construct a logical system of norms, next to his findings about the nature of states (their impermeability and currently their penetrability) and the complexities of nuclear foreign policies and finally to his study of the accelerating rush of mankind through a multitude of threats to survival is a chronicle of the liberal realist responding to successive crises. If he begins as a determined legal idealist, he ends as a despairing and anguished romanticist desperately clinging to Rene Dubos' axiom, "Trend is not Destiny." If the radical transformation of norms, attitudes and policies for which Herz calls is impossible, mankind may indeed have no future. It is also true that he has increased the demands he places on foreign policy and has broadened the agenda of political realism in his later writings. The respectful critic cannot but ask if Herz's despair as his lofty visions have failed is the result of his having abandoned the essence of political realism. Or does his latter-day world view with its stress on world-wide forces of decay foretell the probable end of civilization?

It may be fair to suggest that if Herz were to give his planetary humanism more concreteness, and as a liberal realist design a steady and more practical incremental course for individuals and nations to follow in feeding themselves, holding down population growth and keeping environmental deterioration in check, we might even at this late hour understand more clearly how to move from where we are to where he would have us go.

KENNETH W. THOMPSON

University of Virginia

The United Nations System: Coordinating Its Economic and Social Work. By Martin Hill. (New York: Cambridge University Press, 1978. Pp. xv + 252. $31.50.)

For the past 30 years, the economic and social program of the United Nations has gradually expanded in size, complexity and diversity. Agencies have proliferated and have often been granted a degree of autonomy which provides immunity from effective central control. The problem of coordination has been compounded, not only by the autonomy and proliferation of new global agencies and special funds, but also by the necessity to work with and through regional organizations and country programs. The Economic and Social Council has been poorly equipped as a central coordinating body because of size, lack of authority, and faulty organization. Attempts to fill the void and to bring coherence to the system through administrative consultations and committees have provided only partial solutions to the myriad problems of coordination in a decentralized and fragmented system.

Since 1965 the General Assembly has taken the lead in instigating studies for the purpose of restoring a degree of coherence and unity to the economic and social aspects of the programs of the United Nations family of agencies. By 1969, two reports, one by an Enlarged Committee for Programme and Coordination of the Economic and Social Council, and a Capacity Study of the United Nations Development Program, provided recommendations for the first modest steps in restoring a degree of order out of the existing chaos. In the 1970s, the tempo of interest and concern quickened. The sixth and seventh special sessions of the General Assembly and the pressure for a New International Economic Order focused attention on both the needs of the developing countries and the inadequacies of the United Nations' machinery and programs. In 1974–75, the Economic and Social Council received observations and recommendations from the Secretary-General and the executive heads of the specialized agencies concerning more effective coordination of their programs.

This book by Martin Hill is a final version of a study prepared under the auspices of the United Nations Institute for Training and Research (UNITAR). The preliminary study was submitted to the Economic and Social Council in 1974 and was made available to a committee of 25 experts who, in 1975, produced a report entitled, "A New United Nations Structure for Global Economic Cooperation." This report is the most significant contribution to reform of the UN economic and social system since the charter was written. It proposes the reorganization of the program of the Economic and Social Council, the consolidation or abolition of several committees and agencies, and the creation of a new post of Director-General for Development and International Economic Cooperation.

Hill's study is a valuable companion to the committee's report. As a member of the Secretariats of the League of Nations and United Nations since 1927, he was at the heart of many of the problems of economic and social coordination which he analyzes. Before retirement, he was Assistant Secretary-General for Inter-Agency Affairs. He writes from a depth of experience and appreciation of needs and problems.

In his postscript, Hill compares his study with the report of the committee of experts. While many of the conclusions are similar, Hill's study is more limited in scope and deals more with attitudes and practices rather than with specific recommendations for structural changes. Hill constantly stresses leadership and personal relationships as keys to effective coordination. His book also is rich in historical development of the system. Of the six appendices, the most valuable is a summary of the conclusions and recommendations of the committee.

The study is thorough and accurate, but, because of organization, often redundant. The style is heavy and sentences are frequently convoluted. However, for the student of international organization, the book represents the most comprehensive analysis to date, from the perspective of a knowledgeable international civil servant, of the key area of coordination of the United Nations economic and social activities.

A. LEROY BENNETT

University of Delaware

Social Change in the Capitalist World Economy. Edited by Barbara Hockey Kaplan. (Beverly Hills, Calif.: Sage Publications, 1978. Pp. 239. $18.50, cloth; $7.95, paper.)

This collection extends the socialist analysis of the political economy of the world system. The major text is Immanuel Wallerstein's *The Modern World System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century* (Academic Press, 1974). Related work continues in the journal *Review*, in the Political Economy of the World System *Newsletter*, in the present volume's forthcoming sequel: *The World-System of Capitalism*, edited by Walter Goldfrank, and in Immanuel Wallerstein's *The Modern World System*, Vol. 2, as well as his *The Capitalist World-Economy*, both forthcoming in 1979.

This analysis deserves the political science community's closest attention. It fits the larger intellectual move of unseating the sovereign subject or disaggregated particular instance, by recognizing the overarching centrality of *structure* or *system* or *code*. In studying development or comparative and international politics, beneath the level of appearances lie the structured relations of a capitalist world system, often operating behind the backs of the states as social actors.

Models of accumulation need to embrace the world system in which capitalism has historically developed, and which has created an international stratification of political units into core, peripheral, and semi-peripheral states. As an interdependent and self-contained system, its dynamics are internal, grounded in a complete economic division of labor. Differentiation is the leitmotif: polarization, spatial hierarchization, unequal exchange, unequal distribution of certain types of economic activities and state structures, class formation at the global level, etc.

Market relationships constitute a central dynamic. By creating risks and opportunities, they organize decision-making criteria for economic and (therefore?) state actors. Position within the world economy is hypothesized as determining the distribution of competitive advantage in production, the pattern of economic differentiation, class relations, form of labor control, flow of the surplus, and strength of the individual administrative apparatuses of states.

Familiar shibboleths of political science are attacked. First, nation-states no longer appear as autonomous, self-contained systems, nor as entirely appropriate units of analysis. They are not separate systems of economic production but parts of a single system of production containing multiple political sovereignties. Second, older "ontogenetic" theories of development seem beside the point. Not all countries are capable of following some single path from underdevelopment to self-reliant independent development, no matter what external obstacles to growth are removed. (Notice how this view today inspires the reformism of proposals for a New International Economic Order.) Figure becomes ground; what once seemed like external obstacles now becomes the constitutive system itself.

The ten articles included here extend, systematize, and abridge such a conception. Some of the questions raised concern alternative conceptions of the state, the role of domestic political coalitions, the sources of fascism, of revolution, of counter-revolution, and of modes of core/periphery relationships.

For example: Skocpol and Trimberger, in a comparative study of revolutions, stress the need to complement a world system analysis with comparative historical studies of class relations (for example, the role of the peasantry), the relative autonomy of states (which have a logic of their own), and the international military rivalries among states. International pressures may determine the outcomes of revolutions which bring about more centralized and bureaucratized state structures which in turn give societies a stronger position from which to cope with the international pressures.

Richard Rubinson asks what allows states to move into the core (and achieve major national economic transformation). Along with more and more scholars, he looks at possibilities for building specific political coalitions that could back up or "constitute" such policies. In a careful analysis of Germany and the United States from 1840–1880, he *internationalizes* the stress on political coalitions. Such political opportunities are not merely domestic products; they are a function of conditions in the world economy and the place of a nation within that system.

By stressing the autonomous concern of state actors for preserving attractive investment conditions ("business confidence"), we can drop assumptions about the intentionality of a dominant and unified class. Fred Block extends this theory of the state, but with an added twist: making claims for the self-interest of state actors, resulting even in odd similarities with bureaucratic models. Here, alternative characterizations of the state shed light on the debates about inter-imperialist rivalry (which sometimes hide under the topic of "coordinating the foreign economic policies of advanced industrialized states"). Block's stress on state managers' self-interest best explains conflict resulting from pushes for renegotiating the international rules of the game.

Like innovative structuralisms in other disciplines, these analyses have their limitations. Dotting these conceptions are methodological land mines. We are fruitfully led to ask questions about synchronic analysis, about the stress on exchange relations rather than productive relations, about the theory's ability to account for historical change, in a systematic way, about the neglect of the relations *between* classes domestically and their sources (since institutionalized patterns do not arise, teleologically, "in order to" maximize market opportunities). As usual, to the global "syntactical" relations, we will need to add domestically grounded "semantic" configurations. Without greater sensitivity to these domestic referents, we may be left only with a relatively abstract and static schema of grammatical imperatives. (This has also been a failing of much of the early work on international "regimes" and "regime change.") Even so, from now on, substantial intellectual achievments—in, for example, international political economy, development, or comparative politics—will likely be "post-structuralist." We have come this far.

BRUCE ANDREWS

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Economic Issues and National Security. Edited by Klaus Knorr and Frank Traeger. (Lawrence: Regents Press of Kansas, 1977. Pp. vii + 330. $7.50, paper.)

This interesting new book poses the question: "Will interdependence lead to conflict?" and answers it essentially in the affirmative. The argument subscribed to by most of the contributors is stated in the first chapter by Klaus Knorr. He notes that economic interdependence entails vulnerability of national economies to foreign influences and forces. These influences have required and will continue to require painful adjustments on the part of labor, consumers and businesses. But for a variety of reasons, including the weakening of the Protestant ethic, the electorates in democratic industrialized societies have been reluctant to change their life styles or spending habits at the behest of foreigners. Rather, they have tended to demand that governments solve their problems for them, and if necessary at the expense of foreign nations and exporters. This could lead to policies designed to improve domestic economic conditions "at the expense of employment and price stability in the outside world."

The international dimensions of the problem become more troublesome in the light of the continued pressure on domestic governments to provide greater social services and benefits to their constituents. Indeed, this pressure will likely lead to greater public control of economic life and to expanding governmental activities in all industrial countries. Negotiations with the developing world may also operate in the same direction, with governments forced to control commodity prices or energy supplies to deal effectively with outside powers. Ideally, the clash between what domestic electorates will accept and what foreign interdependence seems to thrust upon them should lead to new international and multilateral solutions, with each government partly modifying its claims in response to international considerations. Rather, the contributors argue, weak governments cannot afford to concede to international pressures and may be forced willy-nilly into conflict with one another. Robert Gilpin ably abets this thesis by noting that mercantilism never was entirely superseded by Smithian liberalism and that economic conflict may ultimately be the carrying on of Clausewitzian policy by other means. A number of contributors observe that a "repoliticization" of domestic economic issues may transform traditionally low-politics interdependence issues into questions of high politics.

There are some descants to the dominant melody. Clark Murdoch's chapter presents a useful compendium of measures of domestic economic vulnerability. Two opposed conclusions might plausibly be derived from such data: (1) that states cannot reduce the vulnerability which they currently face; (2) that their very vulnerability will make conflict between them more likely. Ronald Meltzer's chapter concludes that multilateral solutions to the problem of vulnerability and its domestic impact are much preferable to national ones. But he, like others, does not consider them very likely. In a particularly interesting chapter, and one reminiscent of Robert Gilpin's contentions, Cheryl Christensen concludes that the possession of "structural power" has been particularly important to the stability of international political and economic relations. In the past, Britain, and later the United States, exercised structural power (power which changes rules and regimes, which sets the context for future action). Today, on the other hand, not only has U.S. power declined relative to that of other democratic industrial countries, but also new centers of powers, like OPEC, have emerged to challenge the ascendancy of OECD. And there are others in the wings, particularly the Group of 77. Indeed, the demands for a New International Economic Order may make it impossible for OECD and OPEC to reach agreement on their own.

One can certainly accept that the potential for economic conflict and even economic warfare is greater than it has been in the recent past. The question becomes, however, what the longer-term prospects hold. In the past a weak Japanese government, faced with political and military pressures to protect its position in China, attacked the United States and Southeast Asia in hopes of solving its short-term problems. But little thought was given to the long run: could Japan win the war? In somewhat the same way weak governments today may wish to be able to cope with the pressures upon them and to reduce international and transnational influences, but they may realistically be unable to do so. Murdoch's quantitative conclusions point toward continuing vulnerability. Of course it is possible to cut off one's nose to spite one's face (and that is what Japan did), but almost all of the national measures which would improve one's trade or monetary position have international counterparts that reassert the status quo ante with higher costs to trade on both sides. Purely national solutions, then, are not solutions at all. Does that mean then that the vaunted multilateral remedy is more likely than presumed? Probably not, because, as the authors rightly observe, it has been difficult to find a way to share the costs and risks among industrial nations.

What the contributors have failed to do, however, is to consider the realistic alternatives between economic nationalism on the one hand, and a new multilateralism on the other, for that has been the name of the game. What emerges are neither solutions nor trade wars, but informal linkages and rescue operations, where one government helps out another in trouble. The monetary field has seen such partial remedies for more than a decade. Some regional economic nationalism is not all bad, if that is the term we would apply to the relatively high common European tariff. It was, after all, the threatened imposition of this tariff that led to the huge upsurge of international investment in Europe in the 1960s. If the United States now moved to informal controls on Japanese or European exports, the solution would once again be to o'erleap the tariff with international production and investment, benefiting American consumers and labor.

In talking of greater economic nationalism the contributors have tended to slight the domestic constituencies which strongly support interdependence and an open-trading network, and here one speaks not just of the export industries, but also financial interests, and the multinationals who do not want to bring their capital home. To this point, their argument, not that of George Meany, has prevailed with Congress, and the foreign tax credit looks as solid as it ever did. American business does not want to be hampered in its ability to compete with others overseas, and its position has won support.

It is also arguable that the contributors have got their case wrong, at least as it applies to the United States (and the United States is the only democratic industrial country which could afford to tamper with its foreign trade dependence). Knorr and his colleagues point to a "repoliticization" of economic issues and an expanding government activity in private markets as the trend of the future. In fact, may it not be the other way around? "Reprivatization" may be more characteristic than "politicization," and we may find the government moving out of areas of traditional investment and expenditure. It may move away from regulation as well. If tax-reduction forces have their way (and perhaps even if they don't) we may find greater use of monetary as opposed to fiscal instruments, and perhaps a lower government deficit. This could actually make for higher growth, lower inflation, and more stability in the balance of payments. The impacts of transnational interdependence would then not be so divisive domestically, nor would they militate against the demands of the citizenry.

In the 1930s a number of countries could contemplate cutting their links to the international economy and risk an attempt at autarchy. The great imperial edifices of France and Britain mobilized great resources and markets. Japan needed only Southeast Asia as a substitute for her links with the United States. Germany could move to the Ukraine and to the oil fields of the east and south, thereby regaining a temporary autonomy. But such efforts depended upon low domestic growth and dormant international trade. It is not clear today how nations (with the possible exception of the Soviet Union and China) could hope to provide the same insulation from the international economy that a number of them temporarily achieved 40 years ago. Thus the question is not whether international interdependence may not lead to conflict; but "who wants to fight a war that he knows he cannot win?"

RICHARD ROSECRANCE

Cornell University

The Panama Canal: The Crisis in Historical Perspective. By Walter LaFeber. (New York: Oxford University Press, 1978. Pp. xii + 248. $10.95, cloth; $3.95, paper.)

The first words in this book are: "In January 1964, four United States soldiers died, 85 others were wounded, and 24 Panamanians were killed with nearly 200 wounded during four days of warfare between the troops and Panamanian mobs. Most North Americans soon forgot about the killings. Vietnam, inflation, and Watergate pre-empted headlines for the next ten years. But Panamanians did not forget" (p. vii). Walter LaFeber then provides us with a well-written, comprehensive analysis of U.S.-Panamanian relations regarding the canal and its surrounding zone. The book, appropriately published while congressional debates over new treaty arrangements were occurring, represents, in the author's words, "a survey." The book serves as a useful guide, however, through the historical ups and downs of the U.S.-Panamanian relationship.

The book is divided by chapters representing the separate diplomatic phases through which construction and possession of the canal have evolved. LaFeber's emphasis is immediately apparent by his chapter titles that refer to the names of the primary negotiators: "Balboa, DeLesseps, Cromwell," "Roosevelt, Bunau-Varilla, and Taft," "Wilson, Arias, and Roosevelt" "Arias, Remon, and Eisenhower," "Chiari, Johnson and Robles," "Torrijos, Kissinger, and Carter." Much the story told here centers around their political machinations. It is a tale of duplicity, misperception and self-aggrandizement.

The moving spirits who directed the American dream of building a canal between the Caribbean Sea and the Pacific Ocean from Nicaragua to the Panamanian isthmus were primarily self-interested. The three were primarily William Nelson Cromwell, founder of the prestigious Wall Street law firm, Sullivan and Cromwell. Another key lobbyist for the Panamanian Canal was the famous entrepreneur, Senator Mark Hanna. Perhaps the most important of the Panama Canal founders was the corrupt Buneau-Varilla. He represented Panamanian interests during the treaty negotiations leading to the notorious 1903 agreement which leased in perpetuity control over the canal and its zone to the United States "as if it were sovereign." One minor detail was that Buneau-Varilla had nothing less than 40 million dollars to gain from the deal which was almost immediately rejected by the newly established Panamanian government.

LaFeber shows how the terms of the 1903 treaty forever affected Panamanian perceptions of the United States which no amount of technological contribution, administrative expertise or financial largesse could or ever did alter. Throughout all the phases during which the terms of the 1903 treaty were modified—consistently in favor of Panama—there lingered among Panamanians the belief that the United States had cheated them of their national heritage. As this study reveals, Panamanian nationalism is directly linked to this conviction. LaFeber is especially sensitive to the realities of Panamanian life, its racism, class structure and political dynamics, particularly as they impinged upon the negotiations affecting the economics of the zone and the life of American residents of the zone. The American presence tended to exacerbate these tendencies and also to inflame the nationalist desire to be rid of the North Americans. He writes, for example, "Eisenhower's enlightened policies, produced a tragic irony. After some of the 1955 treaty promises were made real, Panamanian workers in the Zone received higher wages; merchants outside the Zone suddenly could sell goods to the North Americans. The Zone provided the major impetus to the country's economy in the late 1950s. Panamanians therefore grew more dependent on the Zone for their own economic survival. Granting Panama concessions accelerated the country's need to break the colonial relationship and control the entire Canal area, or else face economic stagnation" (p. 130).

In the end, LaFeber's study stands as a justification for the 1978 Torrijos-Carter Treaties. The basic fact is that the canal, at a time of transcontinental transportation, has become increasingly less significant economically for the United States. Its major users today are the Central American states themselves. As LaFeber points out, virtually "10 to 17% of the United States ocean-going commerce, representing less than 1% of the gross national product, uses the Canal" (p. 223). Moreover, in an era of a two-ocean U.S. Navy, the utility of the canal to the defense and security of the United States is marginal. U.S. nuclear submarines, for example, have to surface in order to pass through the canal, diminishing the strategic value of such passage. Several major classes of ships, aircraft carriers, moreover, are too big for the canal. LaFeber describes the machinery of the canal as a "superb antique."

The time had come for the United States and Panama to establish a mature relationship which duly recognized Panamanian sovereignty over the canal and left the Panamanians to their destiny as keepers of the gates and masters of their own fate.

LaFeber includes an excellent bibliographical essay at the end of the book.

EDWARD WEISBAND

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Soviet-American Rivalry: An Expert Analysis of the Economic, Political, and Military Competition Which Dominates Foreign Relations. By Thomas B. Larson. (New York: W. W. Norton, 1978. Pp. xii + 308. $13.95.)

This systematic survey of the Soviet and U.S. political systems should be required reading for every legislator, citizen, and student concerned with world affairs. Written by a former State Department analyst, *Soviet-American Rivalry* examines the strengths and weaknesses, the rhetoric and the behavior of both superpowers, at home and abroad, focusing on the years 1945–1975.

Manageable in size and smoothly written, Larson's book is more readily digested than Paul Hollander's much longer *Soviet and American Society;* it is more up-to-date and less freighted with detail than Brzezinski and Huntington, *Political Power: USA/USSR*, published in 1963.

Though Larson does not explicitly address the convergence thesis (rebutted in Brzezinski and Huntington), he too finds both Soviet and U.S. societies listening to their own drummers; nor does he offer solace to those who hope detente will come to overshadow conflict in U.S.-Soviet relations.

Though the book is well balanced and based on the most reliable and relevant data, Larson's interpretations often tilt to Moscow's polemical advantage. Item: he extols the theoretical potential of collectivism in agriculture, while saying little about the practical contributions of private farming to Soviet production. Item: he praises the narrowing of income differentials in the USSR, but says little about the material perquisites of elite party members and others, and still less about the rise of a black middle class in the United States. Item: he says little about ethnic problems or declining birth rates within the USSR, and less about the possibilities of centrifugal movements within Eastern Europe. Indeed, his survey of accomplishments by both superpowers slights U.S. leadership in culture and other spheres related to the quality of life, e.g., environmental protection.

These shortcomings, however, are minor compared to the book's common sense appraisal of the military and economic balance sheet, an arena often dominated in many U.S. writings by paranoia or overconfidence. The specialist may well prefer the detailed Brzezinski-Huntington case studies and their stimulating theses, but Larson's book meets a real need as Americans debate how to respond to a mounting Soviet challenge.

Any survey of this kind will trigger disagreements, but this characteristic enhances Larson's book's potential for classroom use, not just in international or Soviet studies but as an essential element in any social science course about the modern world.

WALTER C. CLEMENS, JR.

Boston University

Dragon and Eagle: United States-China Relations: Past and Future. Edited by Michel Oksenberg and Robert B. Oxnam. (New York: Basic Books, 1978. Pp. xvi + 384. $13.50.)

This interesting and timely volume has emerged from a 1975–1978 project of the China Council of the Asia Society entitled "Sino-American Relations in Historical Perspective." It is the product of a joint endeavor of 13 China specialists, including the two editors: Michel Oksenberg, Chief China Advisor to the National Security Council, and Robert Oxnam, Program Director of the Asia Society's China Council.

The stated purpose of the book is not to advocate a particular policy but to suggest "a new way of thinking about the relations between the world's most populous society and the world's most affluent society." Their relationship is to be examined in both historical and long-term perspectives. It is within this conceptual framework that the various essays of the volume are organized.

Immediately following the editors' introduction is the section on "mutual perceptions." In the two essays written by Warren Cohen and Tu Wei-ming, attention is given to how Americans and Chinese perceive each other and how their perceptions and images have changed over time in relation to ideologies, politics, and emotions in both countries. The next section deals with the issue of "bilateral interactions." Oksenberg, Lyman Van Slyke, Waldo Heinrichs, and Stanley Lubman examine the various modes of interaction–diplomacy, culture and technology, force, and trade–that have shaped the bilateral relationship between the United States and China.

In the section on "multilateral interactions," the main focus is on the complex quadrangular relationship of the United States, China, Japan, and the Soviet Union. The essays contributed by Allen Whiting and Steven Levine analyze the impact of China's two most important neighbors, Japan and the Soviet Union, upon Sino-

American relations. "Interactions around China's rim" is the topic of the following section. In their respective articles, Peter van Ness, Michael Hunt, and Alexander Woodside discuss Taiwan, Korea, and Southeast Asia as related to American and Chinese interests. The final section of the book is entitled "the legal context." Jerome Cohen's essay shows how international law has been and will continue to be useful in managing relations between China and the United States, whether in cooperation or contention.

By and large, the essays are of high quality and follow a common format. Several of them, Notably those by J. Cohen and Levine, are particularly stimulating and perceptive. For a book of this type, however, there are inevitable problems of overlapping and unevenness. Because of the volume's modest size, some issues are treated rather too briefly to do them justice. This is somewhat compensated for by the inclusion of a useful bibliography to help the reader in pursuing topics at greater depth.

Throughout the book there is a general theme calling for the reversal of the past pattern of misunderstanding and confrontation and the initiation of a new era of reconciliation and cooperation between the United States and China. Unquestionably, the Soviet factor has loomed large and will remain so in Sino-American relations. But, as Oxnam rightfully points out, the "mutually perceived enemy" approach cannot be a solid basis for long-term relationships between the two countries. Instead, "we need to look beyond to other powerful potentials of the Peking-Washington connection—greater economic and diplomatic cooperation among the great powers in Asia; . . . joint U.S.-China involvement in multilateral negotiations on arms control, energy resources, food supply, and the international economic system; and bilateral trade and cultural and scientific exchanges" (p. 44).

Over the emotion-packed issue of Taiwan, the book offers no explicit suggestion for its resolution. By implication, the authors who address themselves to this issue appear to be willing to see Washington accept Peking's three conditions for normalization of relations (severing U.S. diplomatic ties with Taipei, withdrawing all American military personnel from Taiwan, and abrogating the Mutual Defense Treaty with the Nationalist government), while retaining economic and cultural ties with Taiwan in the hope that Taiwan's own military strength and the continuing American and Japanese interests there would deter the PRC from using force against the island.

Whether or not one subscribes to this specific policy stand, *Dragon and Eagle* on the whole is a valuable contribution to the studies of Sino-American relations and deserves serious attention from scholars as well as the general public.

SHAO-CHUAN LENG

University of Virginia

Scarcity, Choice, and Public Policy in Middle Africa. By Donald Rothchild and Robert L. Curry, Jr. (Berkeley: University of California Press, 1978. Pp. viii + 357. $15.75, cloth; $4.95, paper.)

This readable and important study generalizes and advises about economic policy making in what the authors call "Middle Africa." Donald Rothchild (a political scientist) and Robert Curry (an economist) focus on the over 30 states lying between the Arab countries to the north and the continent's white-ruled southern tip.

The book's virtues are many. The authors write for the generalist as well as the specialist, restricting technical economic analysis to appendices following individual chapters. Their arguments are richly referenced, with a near-encyclopedic coverage of the relevant literature on African political economy. Their normative point of view includes informed sympathy for the region's problems, optimism about its potentialities, and yet a pragmatic appreciation of the constraints facing African policy makers. The authors wisely concentrate on a limited number of policy areas—those regarding foreign private investment, external trade, and foreign aid—rather than attempting in one modest-sized volume to analyze and prescribe for domestic economic policy as well as for foreign. However, they might have narrowed their range even further, trading off some loss of scope for greater depth of analysis. The occasional tendency toward superficial treatment is particularly conspicuous following the authors' call, early in the book, for a benefit-cost approach to policy making, based on careful assessment of alternatives.

Rothchild and Curry advocate a "political economy framework" for policy choice. By combining political science and economics, this approach is supposed to inform policy makers as to what is socially desired, economically rational, and administratively feasible, as well as how to assess trade-offs among these values. Theirs is an explicitly "rational-choice" approach, which rejects both incrementalism and any determinism that posits an end-state for African development (e.g., Western capitalism or Marxist socialism). They call, instead, for experimentation which is bold but not naive; they challenge African elites to set "realizable goals" reached through cost-minimizing policies.

The book argues that African states have tended to follow one of three general "strategies of choice" in foreign economic policy. The largest group of states—consisting predominantly of poor, French-speaking countries—is classified as "accommodationist" in that they accept largely intact the foreign economic relations they inherited from the colonial period: openness to new foreign private investment, few restrictions on old or new expatriate enterprise, and reliance on the ex-metropole and its Common Market partners for most trade and aid. Another, smaller group (Ghana, Kenya, Nigeria, Zaire and Zambia) also "accept[s] their structural dependency on the Western capitalist economy" (p. 118). But they seek to reform that relationship, such as through more strictly regulating and taxing foreign enterprises and nationalizing certain sectors or subsectors.

Rothchild's and Curry's third group of Middle African states (Angola, Guinea, Guinea-Bissau, Mozambique, Somalia, and Tanzania) bears the label "transformationist" because they seek "to end their dependence on the Western-dominated economic system" (p. 135). They nationalize and exclude foreign enterprise and work to reduce reliance on foreign aid. Moreover, they are willing to trade off short-term losses in production for the benefits of greater economic independence. Conversely, the other two groups of states have supposedly been sacrificing some meaningful degree of decisional autonomy in order to reap "productionist" benefits (e.g., export industries which expand thanks to foreign private capital and skills). However, the book provides too little evidence of where real gains or losses have occurred in decision-making independence and what their empirical consequences have been. Has skills-scarce Guinea become significantly less dependent on foreign enterprise (private and public)? If so, what net production, legitimacy, and other benefits have resulted? Has Tanzania become any less dependent on foreign aid, or is it that the sources of such aid have been more diverse?

I would hope for more case-study research into the impacts of various degrees of dependence. Such research could assist also in testing the applicability of Rothchild's and Curry's major policy recommendations regarding investment and aid. African states are urged to develop a common set of conditions on which foreign investment would be welcome, "thereby insuring that each government could undertake to protect its national interests without being played off against other governments" (p. 174). However, if no state is permitted to outbid others in fiscal and other incentives, investment would flow largely or entirely according to criteria on which some countries would be permanently disadvantaged: natural resources endowment, market size, and skills level of labor force, among others. All states would protect their virtue, but some would receive little if any new capital and technology unless, as the authors suggest, the same African investment compact could somehow compel multinational corporations to disperse their investments to the benefit of the disadvantaged.

Rothchild's and Curry's chief recommendation on foreign aid also needs further elaboration before being persuasive. They urge the channeling of aid through multilateral organizations which, supposedly, will provide assistance that meets their definition of appropriate aid: "A concession . . . unencumbered by arbitrary restrictions . . . which fulfills the development objective set by decision-makers in the poorer country" (p. 278). However, in the African experience, has United Nations, World Bank, and other multilateral assistance tended to be superior to bilateral aid? Have not donor restrictions at times been useful correctives to the inexperience and/or opportunism of LDC elites?

The book's treatment of foreign trade policy is more thorough and persuasive. The authors present cogent briefs against the feasibility of both regional common markets and non-petroleum commodity cartels and for the viability of common-front trade barrier negotiations with developed market economies.

Whatever may or may not be the deficiencies of its specific arguments, the book's general approach to African economic policy is valuable: largely non-ideological and stressing the assessment of benefits, costs, and trade-offs.

J. D. Esseks

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Understanding Conflict and War, Vol. 2: The Conflict Helix. By R. J. Rummel. (Beverly Hills, Calif.: Halsted Press, 1976. Pp. 400. $17.50.)

A general understanding of manifest, and particularly violent, conflict at the societal level has been much sought by social scientists. In this, the second of an intended five-volume series, R. J. Rummel attempts to provide a more valid and more satisfactory general understanding than has heretofore been offered. In my judgment, he is not successful. On the contrary, in 375 pages of rather laborious reading, he provides very little in the form of new insights or substantive theory and a great deal of conceptual obfuscation. This is not to say that Rummel's argument is lacking in substance and import. There is much here that is pithy. Unfortunately, it does not add up to a sound general understanding of manifest social conflict. What does this volume provide, and why does it fall short of its goal?

In a nutshell, Rummel's argument is that individuals' interests and expectations are the bases of manifest conflict. Interests and expectations are rooted in and largely derived from a particular sociocultural context of meanings, norms, statuses, classes, and powers, which together define latent conflict structures and individuals' perceptions of situations therein. Latent structures of conflict are socially universal because of differences among individuals and because of the existence of organizations (called "anti-fields") among individuals. But not all latent structures of conflict are expressed as manifest conflict. They become manifest, according to Rummel, as individuals become *aware* of their opposing interests and attempt to *create a new balance* of social powers and expectations because of some sudden change in social relationships or circumstances. To understand manifest conflict in general, then, is to understand the social-psychological connection between changing structural elements of sociocultural context and the interests and expectations of individuals within that changing context.

So far, so good. Indeed, as a basic perspective this is an argument far superior to many popular conflict theories. While the latter tend to be simplistic in being based on one or another single causal factor, whether psychological or social-structural, and mechanical in postulating that manifest conflict is caused by certain conditions regardless of societal context, Rummel's argument, by contrast, is neither. It is contextually grounded and integrated across individual and social-structural levels. If the theory incipient in this core argument were adequately developed, knowledge of manifest social conflict would be substantially advanced. And Rummel sets himself on a path to its adequate development. He attempts the construction of an hypothetico-deductive system beginning with the elaboration of basic psychological concepts and progressing through the conceptualization of the elements and structure of sociocultural systems. In this, he contributes some important conceptual/theoretical material. In particular, chapter 17 on social status differences and chapters 26 through 28 on conflict are quite good and well worth reading. In chapter 17, Rummel presents and develops the analytical implications of an argument very important to explanations of conflict based on social inequality:

> The difference in status between two people therefore cannot be an absolute measurement, like the miles between two landmarks. Rather, two people differ by virtue of not only their own wealth and power, but also *as a function of the distribution of these statuses in society.* We cannot gauge the similarity and differences without considering the standards, desires, distribution of resources of society as a whole. . . . Man's location in this social space, his status position, is thus relative to societal context, standards, and desires (p. 138).

In chapters 26 through 28, he presents a conceptualization of conflict—as a structure of opposing dispositions, a situation of opposing interests, and a manifestation of a balancing of powers—that is potentially important to all students of that phenomenon.

But not so much can be said for other parts of the book. The first two-thirds (25 chapters) represent Rummel's efforts to lay out the bases for a general theory of human behavior. Here are presented the premises and conceptions out of which Rummel's arguments about violent societal conflict are purportedly developed in the last third of the book (10 chapters). Unfortunately the lengthy presentation of theoretical building blocks is of little positive consequence as a whole. On the contrary, the reader is beset by terminological overkill, is subjected to a pretense of rigor and precision, and confronts a perspective based on nineteenth-century Aristotelian vitalist philosophical assumptions. The following passage is typical of the material through which the reader must struggle in attempting to come to grips with the purported bases of Rummel's conflict theory:

> Power is first an ontological concept basic to understanding a reality of potentialities and actualities. Our world is a flux of possibilities becoming actual, dispositions becoming manifest, determinables becoming specific. Reality is not only being, but becoming. It is active, a field of potentialities and tendencies struggling against each other to become actual, completed, determinate. . . . What is power within this view? First, power is the linkage between different states of being; between potentialities and actualities, between dispositions and manifestations, and between determinables and determinants (specifics). Second, that linkage is a strength-of-becoming, an active will-to-completeness. It is a push from the level of pure potentiality, of mere possibility, to ever greater levels of clarity and definiteness. Third, it is an imminent energy, an inherent force-towards-identity of all beings. . . . Finally, in essence, power is a *vector-towards-manifestation* (p. 165).

This quotation does more than I could possibly do in a short review to convey the nature and character of the first two-thirds of the book.

And sadly, the aggregation of such passages does not really constitute a solid basis for Rummel's arguments about violent conflict. That is partly owing to the fact that the attempted comprehensiveness and precision of the system of basic conceptions and premises is not accomplished. Instead, the composite impact is of an understandably contorted and sometimes inconsistent admixture of Aristotle, Kant, Schutz, Lewin, Sorokin, and Hegel! But the failure of the first part to provide a solid basis for the second part is due also to the fact that the argument about violent conflict is hardly new or systematic. Rather than drawing directly on the elaborate theoretical structure that precedes, it consists of seven fairly standard hypotheses:

> (1) Conflict manifestations are random regarding specific societal structures of expectations.
> (2) Change produces societal conflict.
> (3) Power shapes conflict.
> (4) Overt conflict has three manifest dimensions.
> (5) Exchange societies manifest pluralistic conflict.
> (6) Authoritative societies manifest communal/traditional conflict.
> (7) Coercive societies manifest elite repression/purges (p. 321).

Such do not require 375 pages of laborious reading to develop!

In all, then, this volume consists of a grand and elaborate theoretical structure which does not produce new or compelling claims about manifest conflict. Rather, it is a pretentious presentation that provides, at most, an interpretive basis for a few common-sense hypotheses which are, themselves, only weakly "tested" in the concluding pages of the volume. The book is not a model of social theory development, nor is it an important contribution to a general understanding of manifest social conflict.

RAYMOND D. DUVALL

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Superships and Nation-States: The Transnational Policies of the Intergovernmental Maritime Consultative Organization. By Harvey B. Silverstein. (Boulder, Colo.: Westview Press, 1978. Pp. xiii + 251. $17.50.)

This book analyzes the organization, decision making and impact of the International Maritime Consultative Organization (IMCO) on international affairs. Participating nations are listed, their ocean-related capabilities are explained, and their negotiating activity is weighed against their capacity for influence in and outside the organization.

The volume is well organized, logical and cohesive. It offers a unique perspective to students of international organization because it employs statistical techniques in a matter-of-fact way without stumbling over theories of world order or paradigms of vote counting. Rather, this is a good old-fashioned case study in modern form that tells us what, how and why this particular organization operates. It is no surprise to learn that the nations with the greatest fiscal interest in shipping lead the organization. After all we have heard about the Third World in the past decade, it is relief to learn that some elements of national power still count for something.

This book will be particularly useful for the policy maker who wants specific questions analyzed and who has little time for seeking answers. It will be less helpful to the theorist because, notwithstanding the author's assurances, there is thankfully little space devoted to integrating the conclusions into the proverbial vast body of wider knowledge. What the book does admirably is answer serious questions about how one important technology-oriented body operates; and in so doing, tells much about the relative importance of technology in international decision making.

Given the importance of the nations with greater financial and tonnage interests in shipping, more attention might have been devoted to the impact of rate-setting conferences on decisions taken by IMCO. Silverstein places the conferences on a line with other organizations—G.O. and N.G.O.—that link to and influence IMCO, but the suspicion remains that private decisions taken by conferences are more important than Silverstein's coding of interactions reveals. Perhaps influence is all the same no matter where it is exercised.

Silverstein makes the most interesting observation that important IMCO decisions are made by specialized personnel with particular interests in shipping and not by traditional foreign offices. On top of this, the very dynamics of the organization itself count for something. Still, these are seen as less salient than the influence of the dominant members. One wonders whether or not the degree of professionalism that Silverstein found in IMCO accounts for the relative good this body has accomplished in serving international shipping as opposed to those UN bodies which seem to run more on radical rhetoric than common sense.

Something must be said about the physical nature of the volume. How sad it is that a highly trained but apparently impoverished Academe is grateful to be published this way. The book is a do-it-yourself, photo-reproduced job which just looks sloppy. Many of Silverstein's charts lose their informational value because they were photographed from a typed page instead of being professionally drafted. Moreover, the price is outrageous considering the poor quality of reproduction and the uneven margins! The publisher, of course, will protest that the cost of publication is high and the sales potential of scholarly works is low. If that is true, perhaps scholars should do some serious reflecting on their attitudes toward publication and its vaunted place in achieving recognition and advancement. What, in fact, is being rewarded: scholarly merit or marginal fiscal approval granted by publishers?

Silverstein has achieved something worthwhile, however; he has clearly described an international organization and explained its actions. Aside from the professional necessity of excessive citation, this is a lean, well-written and highly informative book. When you have finished reading it you will know more about how functional international organizations work and that is worth a lot.

DONALD E. MILSTEN

Maryland Energy Policy Office

The Limits of Military Intervention. Edited by Ellen P. Stern. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 399. $25.00, cloth; $8.95, paper.)

The twelfth volume in the Sage Series on Armed Forces and Society, this is a work prepared with the exquisite care taken by Field Marshall Montgomery before Alemein. First, there was a planning conference to outline issues; second, a conference to receive and debate papers; third, a seminar to evaluate the papers once again. From this elaborate procedure emerge 18 chapters, mostly by political scientists who address an issue of the day in the language of the hour (e.g., "players" handle "options").

The impression that this is a period piece of American social science is strengthened by the scope and focus of the book. With a few notable exceptions, the time reference "is to the immediate present and the years ahead" (p. 9). This eliminates France's role in the American revolution, Italy's in the Spanish Civil War, and America's in the Chinese Civil War. Absent are the rich historical and comparative data encountered in Walter Lacquer's recent work on guerilla warfare (*Guerilla: A Historical and Comparative Study*, Boston: Little, Brown, 1976). Moreover, although mention is made of opportunities and rewards, the emphasis is on limits and restraints. This is defended as an analytical device but it also reflects the chastened mood of post-Vietnam citizen-researchers.

Nonetheless, the quality of individual contributions is high. Roger Hamburg writes perceptively of Soviet views about intervention and about American motives. In a concise essay on military demonstrations, Maury Feld contrasts the classical desire to show the flag with more recent impulses to conceal it behind covert operatives.

A sampling of other interesting findings follows:

1. A preference for complex and costly weapons, e.g., nuclear super-carriers, could inhibit intervention by encouraging us to keep them in central reserve—from which they would not be moved lightly. Conversely, a preference for a "high-low mix" could raise the odds of intervention by encouraging us to risk cheaper weapons in forward deployment (James Digby).
2. Some liberals fear that an all-volunteer force may encourage intervention by attracting bellicose or wholly manipulable personnel. But the AVF is certain to be costly, therefore likely to be small. The smaller the ready force, the

greater the probability that a president would have to mobilize the reserves to cope with even a modest crisis. But a decision to mobilize reserves raises such political risks that it may deter intervention in any but the gravest crisis (Thomas Fabyanic).

3. Not only are Soviet strategic forces kept at a lower state of readiness than ours (Roger Hamburg) but also Soviet military leaders, drawn mostly from ground forces, doubt the value of overseas intervention. As a result, despite recent growth, the Soviet navy lacks a distant water intervention capability (Michael MccGuire).

4. In the Middle East both the United States and the Soviet Union have been relatively forebearing. Each has supported a client on the strategic defensive to the point of enabling it to restore the balance but not to the point of assisting it to a conclusive victory (Roger Hamburg).

As noted earlier, most authors are skeptical of the likelihood and utility of intervention, Richard Smoke less so, perhaps because he looks beyond great power politics. Smoke argues that the world as a whole is growing more intervention-prone, and that overt military action "generally succeeds against opposition currently in existence at the time of the interventory act" (p. 29). Thus the United States stopped North Korea before China entered the war. But Ellen Stern, the volume's able editor, states that the limitations "are increasing" (p. 24). MccGuire argues that major countries have succeeded only when they exerted force in continuous areas (Cuba, Hungary) where both their incentive and power were great. Morris Janowitz asserts that "the marginal utility of the military function has declined" (p. 374), and proceeds insightfully to explore the implications for military morale and civil-military relations.

Other authors doubt the utility of military aid and covert operations. Aid, Caesar Sereseres states, cannot be counted on to enhance security, protect private investment, or promote internal stability. In the long run, American aid to Saigon probably promoted instability by decreasing our leverage for reform. Covert operations, Paul Blackstock contends, too often end by sabotaging official policy, forcing the hand of policy makers, or deluding them by generating biased reports from the field.

Implicit in most such analyses is a scenario in which a great power, usually the United States, confronts a Third World crisis. While a few writers stress technical difficulties (e.g., Third World opponents are increasingly likely to possess precision-guided munitions or nuclear arms or both), most stress political constraints at home. Domestic priorities appear more urgent. The fear of external threat diminishes. Guilt-ridden egalitarianism replaces the cultural and religious arrogance of yesteryear.

Curiously, one contributor whose evidence appears not to support this assessment is John Mueller, whose essay deals most directly with American opinion. Mueller observes that support for intervention has rebounded from the low point it reached just after Vietnam. On the other hand, his discussion of this point does not distinguish the general public from the better-educated strata from which national leaders are drawn. The better-educated are more skeptical towards intervention, just as they are more skeptical—a point Mueller *does* make—towards defense spending. Domestic political constraints, one must conclude, will remain weighty.

So will foreign political constraints. Neutrals get fussier about rights of passage and overflight. Beneficiaries and victims are less obliging than in the salad days of colonialism. Chiang, Rhee, Thieu, and Park are unlikely to be the last clients reluctant to follow our advice. Hanoi remains an inspiration for regimes more willing to accept pain than we are to inflict it. None of this proves that intervention will become a minor-league sport, but it suggests some of the inhibitions under which the great powers will labor.

LAURENCE I. RADWAY

Dartmouth College

War and Domestic Political Violence: The American Capacity for Repression and Reaction. By Michael Stohl. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 153. $14.00, cloth; $6.95, paper.)

This small volume—the outgrowth of a doctoral dissertation—is an empirical study of the impact of five wars on domestic violence within the United States. Michael Stohl selects a series of domestic violence indicators that reflect the two main dimensions of "issue area" and "systemic intent" and examines the trends in these variables before and after the Spanish American War, the First and Second World Wars, Korea and Vietnam. The research design follows the format of the Campbell and Stanley "interrupted time series" version of quasi-experimental designs and uses the standard single and double mood tests together with two Walker Lev tests to determine whether or not the war in question did indeed have an effect on domestic violence. These tests are done for the onset of the war and the conclusion of the war; i.e., the "experiment" that interrupts the time series is first analyzed as the onset of the war and then analyzed as the conclusion of the war. The results show that there are significant intercept changes in domestic violence both with respect to the onset and conclusion of a war; that is, the war "experiment" does make a difference. For the onset of World War I, and the wars in Korea and Vietnam, domestic violence rises, while for the onset of the Spanish American War and World War II it decreases; for the conclusion of the Spanish American War, World Wars I and II, and the Korean War domestic violence increases. To shed further light on these analyses Stohl examines three additional hypotheses concerning wartime economic and social changes.

The volume is generally well written although it contains the typical introductory chapters (three in this case) which review literature somewhat related to but only tangentially integrated with the following research design and analysis. Thus, following an introduction, the two subsequent chapters describe the conflict literature à la Simmel and Coser and some empirical analyses on the linkage between domestic and foreign conflict. While the overview provided is concise, its explicit contribution to what follows is left to the reader. The discussion of research design, analysis and results is ably presented, though some may find the description of the indicators and variables too brief.

All in all, this is a nicely executed study of a significant question that has not received adequate research attention. While a number of scholars have examined the impact of domestic violence on foreign violence, too few have turned the question around to ask about the impact of foreign violence on domestic unrest. This study makes an important contribution towards the understanding of this problem.

DINA A. ZINNES

Indiana University

The Defence of India: A Budgetary Perspective of Strategy and Politics. By Raju G. C. Thomas. (Delhi, Bombay, Calcutta and Madras: Macmillan, 1978. Pp. vi + 245. Rs. 55.00.)

This book will interest students of Indian politics and scholars concerned with the foreign policy of India as it affects and is affected by the balance of power among nations of the Indian Ocean basin. Raju Thomas states that "the size and spread of the annual Indian defence budgets form the theme" (p. 4) of his inquiry. Indeed, the greater part of the analysis is devoted to a careful examination of those factors which determine how much of the Indian budget is alocated to defense as well as to those factors which affect its distribution to the Indian army, air force and navy. What is perhaps of broader interest to policy analysts, however, is the implication inherent in the book's argument: that budgetary constraints and considerations shape a nation's foreign policy and its strategy in pursuing that policy.

The *Defense of India* is divided into eight chapters. An introductory chapter details the substance, but not the argument of the work. Three subsequent chapters are devoted to an examination of macro-level variables which determine the size of defense allocations while another three chapters explore micro-level distributive variables important to the determination of appropriations for each of the Armed Services. A final chapter deals with Indian defense planning in the Post-Bangladesh phase.

Thomas stipulates that there are three macro-level variables important to the politics of allocation. The first of these is labeled perceptions of threat. Exactly what those perceived threats are depends upon the activity and military posture of potential adversaries, namely, Pakistan and China. Basing Indian defense needs on the actions of possible opponents is highly complex. For example, according to the author, Chinese efforts "to secure itself from a more powerful Soviet Union" (p. 8) poses a threat to India because China could also use those arms against India. India's policy of non-alignment was developed in order to fill the gap between *needed* military capability and *actual* capability" (p. 9), as in the case of China.

A second allocative variable is the political structure with primary focus on the Indian parliament. Due to the strength of India's Congress party during the period of this study, the opposition had relatively little impact on defense questions, allowing the Executive or Cabinet a very strong hand in the policy-making process. As in many democracies, Indian defense decisions are greatly influenced by political personalities and bureaucracies. India lacks a large-scale private defense industry so that lobbying by a military-industrial complex is absent from Indian defense politics.

The third variable pertains to the economic constraints and consequences of defense spending: in what way will defense spending retard or abet economic development? This question can be broken into two parts: what resources are available for defense and how much of the available resources can be allocated to defense without wreaking havoc upon the economy? Thomas indicates that the real debates among policy makers have concerned the latter dimension of this two-part question.

The second theme of the book concerns the determination of how monies will be distributed among the three departments of defense. Thomas argues that policy at the micro level depends on four factors: (1) projections about the nature and duration of wars; (2) who is perceiving the threats; (3) personalities of the service heads and their impact upon the Cabinet; and (4) the economic viability and tradeoffs among the types of weapons systems proposed by the three services.

Historically, the Indian army has received the bulk of India's defense allocation. This is due to factors stemming from the British Colonial period. Also India has in the past fought only land wars, thus limiting the need for a large navy. Finally, investments in the army are attractive because the army is labor-intensive whereas both the navy and air force require large amounts of capital for sophisticated weapons, necessitating expenditure of scarce foreign currency reserves and/or dependence on foreign suppliers.

In the course of his examination of the size and shape of the Indian defense budget, Thomas demonstrates that defense planning always prepares a country to fight a past war but leaves it vulnerable in the prosecution of future wars. Thomas implies that India's role on the subcontinent as well as its future needs are now uncertain because Pakistan is no longer as serious a threat to India's security and China is preoccupied elsewhere. With the 1971 withdrawal of the British East of Suez, a power vacuum was created in the Indian Ocean. What will the Soviet Union and the United States do to fill that vacuum? Will the oil- and dollar-rich Moslem nations of the Mideast form an alliance with Pakistan? These are the questions currently of concern to Indian defense planners.

The Defense of India is a solid and useful work. It has two important shortcomings, however. There is a lack of theoretical development, which results in an overly descriptive and at times repetitive account of the policy-making process. Thomas clearly states that he is looking at defense from a budgetary perspective without clearly denoting what that perspective is or why it is to be preferred to other modes of analysis. The second failing of this work results from the inability of the author to obtain information and interviews with key decision makers. Thus, the analysis of the role of various individuals and interest groups within the Indian defense establishment is superficial and incomplete. Finally, two minor complaints. This book could have benefited from editing to rid it of some of its typographical errors, i.e., "Indan" (p. 1). Secondly, for readers not familiar with Indian monetary terms, some indication of the meaning of the words "crores" and "laks" would be useful. The failings of this book do not detract seriously from its worth. They are more in the nature of sins of omission rather than commission.

STEPHAN J. COHEN

University of California, Davis

U.S.-Japan Relations and the Security of East Asia: The Next Decade. Edited by Franklin B. Weinstein. (Boulder, Colo.: Westview Press, 1978. Pp. xii + 318. $14.00, cloth; $7.00, paper.)

This book emanates from the Stanford University Project on United States-Japan Relations which, since 1974, has held discussions on security and arms control problems among non-governmental specialists of both countries. Strategies for the defense of Japan, the role of nuclear deterrence and the Self-Defense Forces (SDF), Korea, arms control and nuclear proliferation receive detailed consideration in this volume. Notably missing is any article specifically concerned with the role of China or the Soviet Union, an omission which, in the case of China, we are told, is derived from the views of Japanese participants about arms control.

For all the past and impending changes in the context of the security relationship (set forth in a contribution by Weinstein and John W. Lewis), remarkably few significant modifications are recommended. No voice is raised for ending or even revising the security treaty, for eliminating the SDF or for a major expansion of it. The American deterrent is deemed sufficiently credible to deal with any nuclear threat, whereas no independent Japanese capability could be (Morton Halperin); there is no need for Japan to abandon her three non-nuclear principles (Tokuya Kubo). Recommendations for greater use of Japanese economic power, for more coordination and for exchange of intelligence information do not represent new departures.

Perhaps the change most clearly reflected here is the emerging consensus in Japan on foreign and defense policies. Acceptance of the treaty and of the SDF at roughly its present level of strength has been growing in spite of the declining fortunes of the Liberal Democrats. With the recent agreements concluded with China, and with the Chinese even encouraging support for the security alliance, China policy seems to have become a part of the consensus.

Differences among the participants are those of emphasis and degree rather than of kind. Should Japan increase its defense forces or rely on a non-military approach to security? Elements of both are needed, according to Makoto Momoi, while Henry Rowen urges strengthened self-defense, especially of air and naval forces. Weinstein takes strong exception to what he terms the "fallacies of burden-sharing." James Morley's view is very balanced: if the United States has overemphasized the military approach to security, Japan has slighted it. A security policy needs to be based on such political realities as American acceptance of very limited Japanese rearmament and Japanese recognition of the need to provide convincing evidence of its determination to defend itself. The recent move to increase substantially Japan's contribution toward the cost of maintaining American bases represents a useful step in accord with those realities.

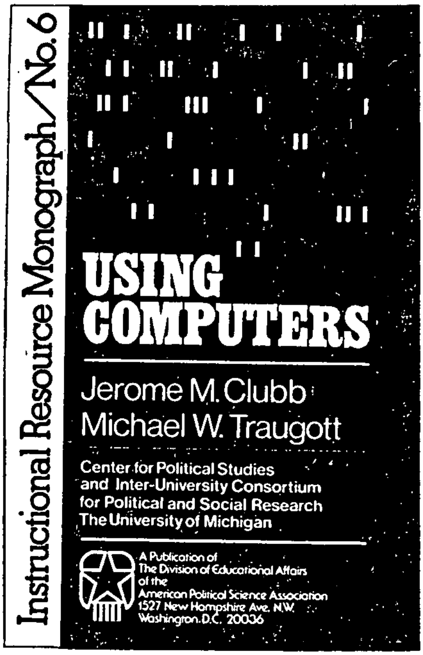
The editor argues for a phased withdrawal of all American forces from South Korea, a policy which he feels can find support in Japan. Other proposals advocate cross-recognition by the four major powers (Fuji Kamiya), and unilateral withdrawal with a symmetrical policy toward both North and South (Selig Harrison); the drawbacks of the latter proposal are pointed out in a critical commentary (by Ralph Clough). Again, the differences are ones of implementation rather than basic policy. No one sees war as imminent or questions Korea's strategic importance. Ultimately, one gets down to the matter of judgment and differing perceptions of the drives, motives, probable reactions of the North Korean regime, in particular, its leader.

The discussion of arms control and nuclear proliferation by Ryukichi Imai illustrates the problems caused by differing perceptions. At least until recently, the Japanese have not perceived this as a subject warranting serious attention; their approach has been from a regional, not global, perspective, and nuclear energy bulks large in their calculations. Other, more general problems of differing perspectives are indicated by Daniel Okimoto.

The quality of these individual presentations is uniformly high, although coverage and treatment of issues is uneven. If the range of opinions is limited—the neutralist position of orthodox Japanese socialism is not present, for example—this reflects the widening consensus noted above. A decade ago, the focal points of argument undoubtedly would have been different. For the present, the Japanese-American security relationship appears in sound shape. To sustain it will require continued attention to economic controversies lest they become politically divisive and to the pervasive sources of misperception and misunderstanding which can undermine both political and economic ties.

WILLARD H. ELSBREE

Ohio University



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